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HYPNOTHERAPY IN HABIT DISORDERS[1]

by Herbert Mann, M.D.[2]

Hypnosis, an art of interpersonal relationship, is a realistic, comprehensive approach to the total patient. It is unfortunate that a few misguided vociferous antagonists blatantly wag their woeful tongues in warnings against the fantasied dangers of hypnosis as an unorthodox and unconventional therapeutic technique. Throughout the history of medicine self-styled authorities, reacting to their own fears and prejudices, have tried desperately to maintain the status quo, to resist change in accepted concepts, to impugn those who challenged principles and tenets that had begun to assume the aura of religious fanaticism. Not very long ago psychiatry and psychiatrists were looked upon with suspicion and were being castigated and ridiculed for the strangeness of their theories, principles, and practices. Dire warnings of the dangers of psychotherapy and psychoanalysis were being publicized. More recently, unjust and unwarranted criticism has been directed toward hypnosis, hypnotherapy, and hypnotherapists. In spite of encouragement by the American Psychiatric Association for the general physician to accept and utilize psychotherapeutic principles and techniques, unconscious resentment on the part of a few psychiatrists has resulted in unfounded, unjust, and fallacious criticism of hypnosis. Incongruous insistence on strict adherence to old, timeworn, hoary theories, fantastic psychoanalytic interpretations, and inordinate emphasis on the dangers of symptom removal have encouraged a revolt among that large majority of independent, thinking clinicians who are not afraid to scrutinize soberly and to dispute the credibility of accepted dogma.

For too long a time we have been shackled by the false doctrine that symptom removal is dangerous because by some magic formula and alchemy a more seriously debilitating symptom will appear to take its place. Such generalization has no basis in fact. The few case histories repetitiously cited as proof of this false theory are interesting and illuminating examples of poor discipline in scientific observation. The opponents of hypnotherapy and of symptom removal still adhere to archaic principles and interpretations so aptly and ridiculously illustrated by the case of the ulcer patient who habitually drinks milk at frequent intervals, not to relieve his organic distress, but to demonstrate his resentment and hostility toward his mother, who deprived him of the breast in early infancy, or by the psychosis that is precipitated due to rejection of mother following excision of the navel in umbilical herniorrhaphy. If we meekly and passively accept this type of fantastic interpretation, we should be consistent and acknowledge the fact that the psychiatrist who habitually vocalizes and verbalizes his fears of symptom removal is reacting to his own latent psychologically traumatic experiences. The psychiatrist who so vehemently castigates the use of hypnosis in symptom removal epitomizes the ancient axiom that psychotherapy implies a relationship between two people, one of whom is anxious, preferably the patient.

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[1] Presented at the Third Annual Scientific Meeting of The American Society of Clinical Hypnosis, Miami Beach, Florida, August 9, 1960.

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The fantasied dangers of symptom removal are based on illogical post hoc observations. The case of the taxi driver (1), whose bruxism was relieved with the use of hypnosis and who some time later awakened during the night in the act of choking his wife, is cited as proof of the danger of hypnosis and of symptom removal. The fact that a temporal relationship was evident apparently was sufficient proof for a learned psychiatrist to accept, a priori, a causal relationship. The business executive's wife who became a compulsive eater after being cured of her nail-biting is another case dramatized by the same psychiatrist to presumptively prove the danger of hypnotic symptom removal (1). Peculiarly enough, his cases always substitute more serious and debilitating symptoms for those that have been removed, while a serious and respected authority, Wolberg, believes that only occasional patients, usually hysterics, may substitute a symptom with the same dynamic significance but perhaps less incapacitating. Wolberg (2) also states that no symptom will be abandoned which serves as a defense against intense anxiety or as an important way of adjusting the patient to his life situation. Erickson (3) has emphasized the fact that the great difficulties involved in producing changes in the personality of a desired therapeutic character make evident the illogic of assuming that the time- and situation-limited hypnotic trance can bring about significant harmful effects when earnestly desired beneficial effects are so hard to achieve.

The application of hypnotic techniques to the treatment of habit disorders should not preclude the usual history-taking, physical examination, and any laboratory procedures that may be indicated. With the exception of patients whose history, personality profile, or physical examination indicate that they are in need of other types of therapy, hypnotic techniques can be applied advantageously. The successful hypnotherapist establishes rapport with his patient and elicits his complete trust. Such a doctor-patient relationship often proves therapeutic in itself, relieves anxieties and tensions, and aids in securing essential information and in obtaining co-operation from the patient.

Included among the habit disorders that can be treated with hypnotic techniques are nail-biting, insomnia, tics, stuttering and stammering, perverse eating habits, and inordinate smoking and drinking. Therapy should be predicated upon acceptance of the patient's behavior without criticism, scorn, or ridicule. A sympathetic, understanding approach to the patient and his problem is prerequisite to successful therapy. The physician must be confident in his ability to cope with the problem and have faith in the validity of the therapeutic technique he utilizes.

It must be understood that hypnotherapy is not a magic modality that can be utilized as a bludgeon to belabor the patient into abandoning his habit disorder. In analyzing therapeutic failures, it is readily apparent that some clinicians' over-enthusiasm with hypnosis tempts them to direct prematurely and precipitously a frontal attack upon the undesirable symptom, the objective being prompt and total annihilation of the disorder. This approach is occasionally indicated in emergency situations and is transiently successful in hysterics whose need for an invincible authority figure is so overwhelming that they readily follow the suggestions of the hypnotherapist. But for the majority of patients time should be allocated for a full discussion of the habit disorder, its relationship to the patient's total life situation, the degree with which the patient is pre-occupied with the habit, its incapacitating or embarrassing feat-

ures, and any other emotional problems of which the patient has conscious awareness.

The goal in therapy is of paramount importance. The experienced hypnotherapist utilizes direct symptom removal techniques rather infrequently. The usual goal in symptom removal is to aid the patient to function adequately and to minimize the disabling aspects of his symptoms while still permitting the patient's neurotic needs to be served (4). The patient may comfortably and confidently accept a goal more readily attainable than complete symptom removal. The needs of the patient may be met through the use of symptom substitution, symptom amelioration, symptom utilization, or symptom transformation. Even minimal improvement in the habit disorder effected under hypnosis may be followed by spontaneous reorientation of the patient's attitude toward his total life situation. Increased confidence in his ability to cope with his problem may then affect his total personality, restore his self-respect, influence his capacity to deal with his symptoms, anxieties, and conflicts and develop ego strength and inner security.

The case of Mrs. M. D., aged 42 years, illustrates a logical, realistic application of hypnotic techniques. The patient verbalized her complaint as one of obesity and could not account for the excess weight of many years duration. She steadfastly maintained that she ate nothing other than a small meal in the evening, which amounted to about 400 calories. She was an intelligent woman, neatly and expensive attired, apparently co-operative, but was adroit in evading questions directed toward consumption of alcoholic beverages. Hypnosis was suggested in order to create for the patient a more protected situation, in which she could feel free to express herself. In the very lightest trance, with eyes closed, the patient readily revealed a long history of alcoholism culminating in her present routine of starting the day off with a bourbon highball, then drinking vodka martinis throughout the day. It was estimated that more than one quart of vodka was consumed daily, plus several ounces of vermouth and bourbon. Food intake was limited to a very small dinner consisting of meat and salad. In therapy, no attempt was made to precipitously remove the habit disorder, not because of any fantasied danger in such an approach, but because a direct attack is almost invariably ineffective. A suggestion was offered that perhaps, upon awakening in the morning, a cup of hot coffee or tea might be substituted for the bourbon highball and that the patient might experience the same stimulating, eye-opening effect from coffee or tea that she had been obtaining from bourbon. Such substitution would also aid her in reducing excess weight by eliminating one source of her obesity. It was also suggested that at noon she might enjoy the stimulating effect of another hot drink, perhaps a cup of bouillon, soup, coffee, or tea. The following week, the patient enthusiastically reported that she had been awakening each morning with no desire for bourbon and had been enjoying a cup of coffee. On two occasions during the week, she had missed several martinis because she took time out for a bowl of soup. The patient now freely and unabashedly co-operated, at a conscious level, in talking about her consumption of alcohol. She readily volunteered the information that several years ago, because of ineffective attempts to overcome her obesity by adherence to rigid rules of diet, she started to drink in order to forget her problem. As alcoholic consumption increased, her appetite for food decreased while her weight remained stable. Suggestions, at a conscious and subconscious level, were oriented around an educative program to

teach the patient proper eating habits. An unlimited-calorie, high protein, high fat, low carbohydrate diet was suggested in order to avoid guilt feelings resulting from an occasional infraction of rigid rules of diet, and to offer a wide selection of food and comparative freedom of choice, thus stimulating co-operation and enthusiasm. No further suggestions were directed at the problem of alcoholism. Over a period of three months, during which time the patient was seen weekly, proper eating habits became well established, while the consumption of alcohol rapidly decreased and was entirely and spontaneously eliminated within the first five weeks. During therapy, it was repeatedly emphasized that the goal was not loss of weight per se, but the acceptance of ideas and suggestions that help in developing eating habits which are effective in removing excess weight and in maintaining optimum weight when that level has been reached. The patient has maintained her optimum weight over a year, has had no desire to indulge in alcoholic beverages and, what is even more gratifying to both patient and therapist, her attitude toward life in general has undergone a startling transformation, so that she now enjoys social activities and recreation and experiences increased confidence in her own ability to meet the problems of life.

In conclusion, it should be emphasized that the fantasied dangers of symptom removal are a reflection of the therapist's inherent fears, his inability to comprehend fully human behavior and motivation, and his unconscious resentment of the non-psychiatrists' utilization of psychological principles and techniques. Psychotherapy will continue to be practiced by all clinicians, knowingly or unknowingly, because the doctor-patient relationship often proves therapeutic in itself (5). Therapy should be directed toward reeducating the patient and effecting a change in attitudes. Suggestions should be so constructed as to recognize the psychological needs of the patient while altering, ameliorating, and transforming the objectionable features of the symptom. Hypnotherapy implies treatment of the total patient; it offers an empirical approach that is ideally adaptable to the frustrated patient who seeks reassurance and guidance in an atmosphere of genuine interest and enthusiasm.

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AN EXPERIMENTAL INVESTIGATION OF HYPNOTIC AGE-REGRESSION[1]

by William E. Edmonston, Jr., Ph.D.[2]

Hypnotic age-regression, the reinstatement of previously acquired but subsequently forgotten or extinguished responses under hypnosis, has been in dispute since its discovery by Krafft-Ebing in 1887. It has only been in the last thirty years that the phenomenon has been subjected to experimentation.

Two theories have evolved which attempt to account for the behaviors which occur during hypnotic age-regression. The first, the role-taking theory, states that the subject plays the role of an age-regressed person as he perceives it. He acts out, to the best of his ability, what he thinks is expected of him by the hypnotist. Thus by this theory subject variables are assigned greater importance in determining behavior than are stimulus variables (words spoken by the hypnotist).

The second, the functional ablation theory, places greater emphasis on what is done to the subject as it affects how he behaves during hypnotic age-regression. Both learned and maturational behaviors, which appeared after the age to which the subject is regressed, are said to be functionally ablated by the words of the hypnotist and no longer accessible as part of the subject's response repertory. This theory is essentially one of verbal conditioning. The words spoken by the hypnotist thus become the stimuli evoking the various behaviors of hypnotic age-regression.

Both theories have had their exponents. Young (1), Sarbin (2), and Orne (3) have offered experimental evidence to support the role-taking theory, while Platonow (4); Erickson (5); Spiegel, Shor, and Fishman (6); Gidro-Frank and Bowersbuch; and others (8, 9, 10) have attested to the genuineness of the phenomenon.

With the exception of two articles, one by LeCron (11) and the other by McCranie and Crasilneck (12), the majority of the studies have concentrated on intellectual or personality evaluations to support their respective theses. Both of these studies used conditioned responses, but the results were equivocal. LeCron supported the ablation theory, while McCranie and Crasilneck found "no neurophysiologic proof of the reality of this phenomenon." (12, p. 123).

It is clear that the evidence has been unable to support firmly either the functional ablation or the role-taking interpretations of hypnotic age-regression. One apparent omission should be considered. In all of the studies cited above the subject's behavior during age-regression was compared either to norms or to their own behavior while simulating a younger age. Only one study (2) included measures on the subjects at the actual age to which they were later regressed. The measurements used, however, were of verbal responses on an intelligence test and could readily be simulated by a co-operative subject. What has been needed is a study which will utilize the attribute of the above work (prior measurement) in an involuntary response context.

The present study was an attempt to evaluate the ablation theory of hypnotic age-regression by utilizing a brief (three to four weeks) regres-

[1] This article is based on a dissertation submitted in partial fulfillment of the requirements for the degree of Ph.D. at the University of Kentucky. The author is indebted to Dr. Frank A. Pattie for his suggestions, advice, and constant assistance.

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sion period with a previously established involuntary, conditioned response as the criterion of authenticity.

METHOD

Subjects

The subjects were 12 male and female volunteers from the nursing and nursing assistant staffs of the Veterans Administration Hospital, Lexington, Kentucky, and the student body of the University of Kentucky, equally divided into experimental (hypnotic age-regression) and control groups.

The experimental group consisted of two female and four male subjects (mean age, 27.3 years) chosen from 23 volunteers. There were two bases of choice. First, each subject was able to achieve a deep trance state as noted by (a) the ability to open eyes and still maintain the trance, (b) amnesia for events occurring in the trance, and (c) the ability to follow post-hypnotic suggestions. One of the latter suggestions was a signal for the rapid induction of a trance. This signal was the lowering of a pen in front of the subject's visual field and counting from one to three. Since the primary concern of this study was a phenomenon which occurs during a deep trance and is not dependent upon induction technique per se, initial induction techniques varied. However, the hypnotic age-regression procedure, as described below, was the same for all subjects in this group.

The second criterion for choosing the experimental subjects was an actual hypnotic age-regression procedure. By means of the rapid-induction signal and a standard deepening technique (counting from 1 to 10) each subject was placed in a deep trance. The subject was told to hallucinate a calendar, the top page of which consisted of the numerals of the year, 1959; the next page beneath this stated the preceding year, 1958; the next, 1957; then 1956; and so forth. He was further instructed to count the pages of this calendar as they were removed, each page being the equivalent of one year. As he thus counted himself back in time, continued suggestions of returning youth, even to the point of his body diminishing in size, were given. Suggestions of voice change, however, were not included. At various age levels the subject was stopped in his counting and asked where he was and what he was doing. Behavior noted during this questioning, and a waking discussion of the individuals' subjective feelings during this procedure determined the selection of the six experimental subjects. (Amnesia for events of the trance was removed for these discussions.)

The control group matched the experimental both in sex distribution (two females, four males) and in mean age (29.8 years). Participation was again voluntary, thus making the two groups more nearly equated with respect to motivation.

The experimental group returned for four more one-hour hypnotic age-regression sessions. During these periods, various behaviors of age-regression were noted in all subjects. All experienced the subjective reality of regression; they felt they were "really there." It is interesting to note that two of the subjects initially felt that they were watching themselves at an earlier age but later gained the actual feeling of reliving. Two other subjects' voices spontaneously changed to a falsetto as they regressed through adolescence, and one stated she felt as if she were taking up less space in the chair when she reached childhood. These behaviors, as well as the subjects' abilities to relive events and recall names and places long since forgotten, were the clinical evidence of the individuals' abilities to engage in hypnotic age-regression.

EXPERIMENTAL PROCEDURES AND APPARATUS

Avoidance-Conditioning Procedure

Avoidance-conditioning involved the presentation of an innocuous stimulus shortly before a noxious, response-forcing stimulus.

A conditioning trial was defined as a single pairing of the conditioned stimulus (CS) and the unconditioned stimulus (US) with a 500 msec. interval. The CS was an audible click and a simultaneous, noticeable increase in brightness in a dimly illuminated white paper disk in front of the subject. The US was a puff of air directed at the cornea of the left eye and occurring 500 msec. after the onset of the CS.

Each subject was placed in head and eye apparatuses (see below), and the following instructions were read:

> This part of the experiment is concerned with testing the natural reactions of the eye to light changes. From time to time you will hear a click, the light before you will change, and you will feel a puff of air strike your left eye. When these things occur, please do not try to control your natural reactions. During this procedure you are merely to look straight ahead at the light, relax, and remain as comfortable as possible. In order to give you a point of reference, it is now —— A. M. (P. M.) and the date is ————————, 1959. (Appropriate times and dates were used.)

The subject was then given 80 conditioning trials, with one minute rest periods after trials 30 and 60.

Recording was accomplished with an electroencephalograph (EEG). One channel was used to record the onset of both the CS and the US by means of a marker system. Each trial was controlled with a double-pole, double-throw, spring-return, toggle switch. When this switch was closed, a click was heard; the light before the subject became brighter; the marker system released a small surge of current, which made a record; and an electronic interval timer was activated. After 500 msec. the marker system again released a surge of current, which made a second record, and a solenoid valve released a puff of air under 180 mm. manometric pressure from a 500 ml. glass flask reservoir. The toggle switch was then released, and the light returned to its original degree of brightness.

A separate EEG channel was used to record the subject's eye-blinks. A small rectangular metal plate was attached to the subject's right eyelid with adhesive tape. A thin, flexible wire, soldered on the eyelid plate, was connected to an ordinary phonograph crystal, which was attached to a head band. Any eyelid movement activated the crystal, generating a voltage, which was amplified and recorded by the ink-writer unit of the EEG machine. A conditioned response (CR) was defined as any vertical deflection of the pen greater than 1 mm. occurring between 200 and 500 msec. after the onset of the CS. Any eyeblink occurring prior to 200 msec. was considered an alpha or beta response (reflex to the light alone) and not counted.

Extinction Procedure

Extinction involved the presentation of the CS without the US. For the purposes of this study, an extinction trial included the presentation of the CS alone and the 500 msec. period following the onset of the CS.

Three to four weeks following the avoidance-conditioning procedure, both groups of subjects (experimental and control groups) returned to the conditioning apparatus. On this occasion they were told to "look at the light as before." Each subject was then given a series of extinction trials. The CR was considered extinguished when 10 consecutive extinction trials occurred without a CR being elicited.

Hypnotic Age-Regression Procedure

With respect to the CS and US, a hypnotic age-regression trial was the

same as an extinction trial. The experimental and control groups were treated differently with respect to hypnotic age-regression.

Experimental Group. Immediately following the last extinction criterion trial, each subject was placed in a deep hypnotic trance state by means of the rapid-induction signal. He then visualized a single-sheet day calendar "in his mind's eye," which was used, as before, to produce age-regression. On this occasion, however, regression was over a period of days rather than years. When each individual reached the day on which his avoidance conditioning was accomplished, he was stopped and questioned as to his activities during the time preceding the actual conditioning. All subjects demonstrated clinical evidence of age-regression to that particular day. (See results.)

The subject was then informed of the time of day (the exact time of the beginning of his individual avoidance-conditioning, though this was never stated, as such, to the subject), and told that he would open his eyes and stare at the light before him. The subject was given 80 hypnotic age-regression trials with one-minute rest periods after trials 30 and 60.

It has been suggested that subjects may require a more prolonged hypnotic regression to return physiologically and psychologically to a prior age. This suggestion was evaluated by means of a second hypnotic age-regression procedure. Following the first 80 trials during age-regression, each subject was returned to his present age by reversing the calendar process, but he was not removed from the deep trance state. After inducing amnesia for the 80 trials just completed, each subject was again age-regressed and the entire procedure repeated for 80 additional hypnotic age-regression trials.

Control Group. Immediately following the last extinction criterion trial, each subject was told to continue staring at the light before him. He was then given 80 extinction trials, as defined above, with rest periods after trials 30 and 60. As with the experimental group, 80 additional trials were given with a brief rest period between the two groups of trials. Thus, a comparison of the experimental and control groups yields some measure of the authenticity of hypnotic age-regression.

RESULTS

Statistical analysis was used to compare: (a) the experimental and control groups during acquisition (conditioning), (b) the experimental and control groups during extinction, (c) the experimental and control groups during the hypnotic age-regression trials,[3] and (d) the acquisition and age-regression performances of the experimental group alone.

Acquisition

The 80 acquisition trials were quartered, and a total frequency count of CRs in the first (trials 1 through 20), second, third and fourth quarters was recorded for each subject. Scores in these blocks of trials were then pooled for the experimental and control groups, and an analysis of variance was applied to the grouped data.

Table 1 shows no difference between the experimental and control groups with respect to the acquisition of the CRs. The significant difference which occurs over the blocks of trials merely states that both groups did, in fact, yield progressively more CRs with an increase in the number of trials; i.e., conditioning occurred.

---

[3] While the control group did not undergo any hypnotic age-regression, these trials were the same for both groups with respect to the CS and the US.

TABLE 1

SUMMARY OF THE ANALYSIS OF VARIANCE COMPARING THE EXPERIMENTAL AND CONTROL GROUPS ON CONDITIONED RESPONSE ACQUISITION

Source of Variance	Degrees of Freedom	Mean Squares	F Ratio
Between Subjects	11		
Groups (G)	1	21.34	.34
Error	10	62.78	
Within Subjects	36		
Blocks of Trials (B)	3	111.50	9.91*
G × B	3	19.83	1.76
Error	30	11.25	
Total	47		

*Significant at the .001 level.

TABLE 2

SUMMARY OF THE ANALYSIS OF VARIANCE COMPARING THE EXPERIMENTAL AND CONTROL GROUPS OF THE TWO SERIES OF HYPNOTIC AGE-REGRESSION TRIALS

Source of Variance	Degrees of Freedom	Mean Squares	F Ratio
Between Subjects	11		
Groups (G)	1	615.09	11.14**
Error	10	55.20	
Within Subjects	84		
Age-Regression Series of Trials (A)	1	7.59	1.22
Blocks of Trials (B)	3	3.48	.56
A × B	3	1.17	.19
G × A	1	46.77	7.52**
G × B	3	18.12	2.91*
G × A × B	3	1.06	.17
Error	70	6.22	
Total	95		

*Significant at the .05 level.
**Significant at the .01 level.

Extinction

The total number of trials-to-extinction for each subject was recorded and averaged for the two groups. The mean number of trials-to-extinction for the experimental group was 32.67; for the control group, 38.17. A *t*-test between the two distributions thus obtained yielded a value of .27 ($P > .70$). Since the number of trials-to-extinction offers a measure of the strength of the response being extinguished, the experimental and control groups are equal in this respect.

Hypnotic Age-Regression

Each of the two series of 80 age-regression trials was quartered, in the same manner as the acquisition trials. A count of the CRs in each quarter was recorded for each subject. Scores in these blocks of trials were then pooled in their respective groups, and an analysis of variance was applied to the grouped data. Homogeneity of the

TABLE 3

MEANS, STANDARD DEVIATIONS, AND *t*-TESTS COMPARING ACQUISITION, FIRST AGE-REGRESSION, AND SECOND AGE-REGRESSION FOR THE EXPERIMENTAL GROUP

Conditions	Mean CRs	Standard Deviations	Conditions Compared	*t* Values
Acquisition (A).....	37.33	16.11	A vs. F	1.84
1st Age-Regression (F)....	22.50	9.97	A vs. S	.55
2d Age-Regression (S)	30.33	19.54	F vs. S	1.34

TABLE 4

REGRESSION LINE EQUATIONS AND *t*-TESTS COMPARING THE REGRESSION COEFFICIENTS OF ACQUISITION, FIRST AGE-REGRESSION, AND SECOND AGE-REGRESSION FOR THE EXPERIMENTAL GROUP

Conditions	Regression Line Equations	Conditions Compared	*t* Values
Acquisition (A)	$Y = .049X - .799$	A vs. F	4.35*
First Age-Regression (F)	$Y = .017X - .974$	A vs. S	4.59*
Second Age-Regression (S)	$Y = .014X - 1.700$	F vs. S	.39

*Significant at the .01 level.

variances involved was demonstrated by Bartlett's test.

Table 2 establishes several important findings. It is clear that the experimental and control groups performed differently during the age-regression trials, the former producing a greater number of CRs. The significant interactions between the groups and the age-regression series of trials, and between the groups and the blocks of trials make this even more apparent. The first of these (G $\times$ A) indicates that the experimental subjects gave more CRs during the second series of age-regression trials than the first, while the control subjects gave fewer. The second interaction (G $\times$ B) bears this out still further in stating that the experimental group tended to increase the number of CRs as the blocks of trials increased, while the control group decreased its number of CRs.

Experimental Group: Quantitative Data

The total number of CRs for each experimental subject was recorded for the three conditions: acquisition, first age-regression, and second age-regression. The means, standard deviations, and *t*-test values for the comparisons of those distributions appear in Table 3, which shows that neither the first nor the second age-regression differed significantly from the original acquisition with respect to the mean number of CRs produced.

Since the above comparison involves only one point on each of the lines which describe these distributions, the mean of the respective distributions, further analysis was necessary. Using the numbers of CRs produced on each of the 80 trials of acquisition, first age-regression, and second age-regression, the experimenter derived the equations of the regression lines which best describe the functions shown in Figure 1.

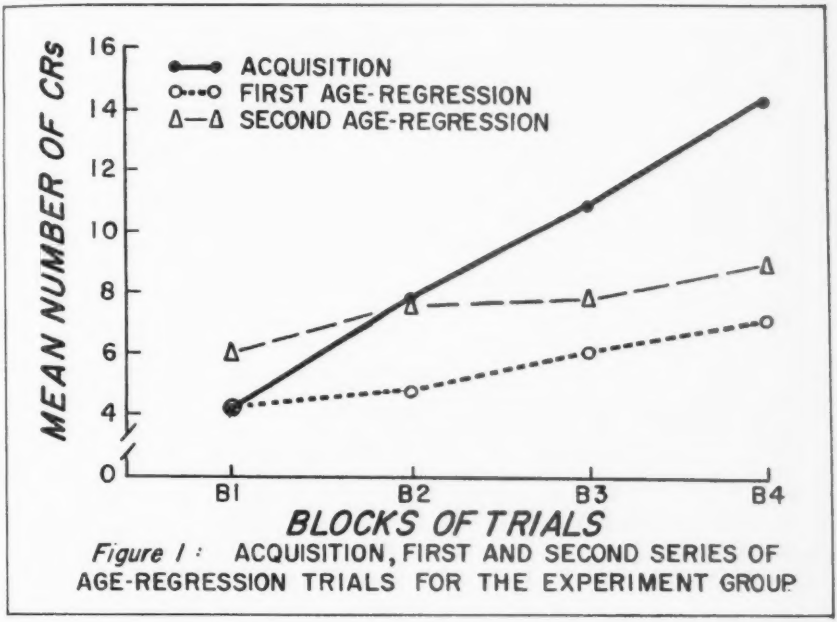

Figure 1 : ACQUISITION, FIRST AND SECOND SERIES OF AGE-REGRESSION TRIALS FOR THE EXPERIMENT GROUP

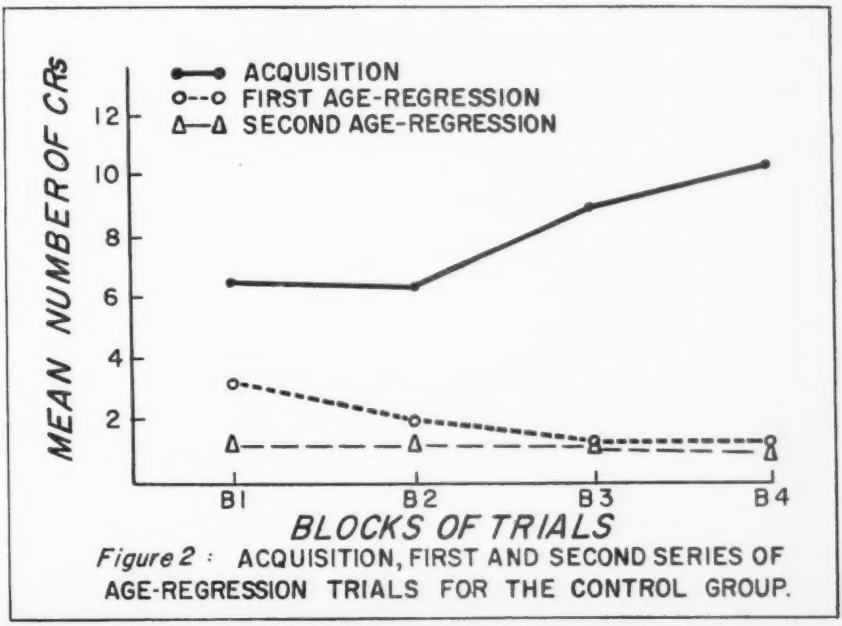

Figure 2 : ACQUISITION, FIRST AND SECOND SERIES OF AGE-REGRESSION TRIALS FOR THE CONTROL GROUP.

(These same functions of the control group appear in Figure 2). These and the *t*-test values for differences between the respective regression coefficients appear in Table 4.

The regression coefficients for both the first and second age-regressions differ from that of acquisition. Thus it becomes clear that, while the experimental subjects produced the same mean number of CRs during each condition, their rate of responding was considerably less during the age-regression series of trials. Thus, hypnotically age-regressed subjects do not perform in precisely the same manner as they did at the prior period in their history to which they were regressed.

The data of the experimental group show that, while there is no difference in the mean number of CRs between the first and second age-regression series, the average of the second age-regression more closely approximates the acquisition average than does the first. This trend is particularly noticeable also in Figure 1 in the difference between the intercepts of the regression lines and the general rise in the second age-regression curve. It seems that the second age-regression not only approximated the average acquisition performance more closely, but approximated the maximum acquisition performance as well; that is, the mean number of CRs in the last block of conditioning trials.

Experimental Group: Qualitative Data

Not all results in studies such as this are quantitative. Qualitative findings are equally pertinent. Therefore, after each experimental subject had been age-regressed to his day of conditioning, he was questioned as to his activities on that day. All of the subjects gave answers appropriate to the day of conditioning.

The three university students of this group correctly named the courses they had attended that morning. Since they attended these classes three days out of the week, such accuracy could be attributed to chance guessing. However, with the remaining three subjects (two nurses and one ward attendant) more reliable evidence was available. These subjects correctly named the ward on which they had been working the morning of the day they were conditioned. One even volunteered that she felt the puff of air during the hypnotic age-regression trials. The puff, of course, had been omitted.

While all of the experimental subjects expressed the subjective feeling of reliving the day of conditioning, one of the students produced the most striking age-regression behaviors. His left eye, the one toward which the puff of air had been directed during acquisition, teared continually during regression, as it had when he was being conditioned. The subject later reported that throughout the entire regression period he was worried about an examination in physics due the next day. This examination actually occurred on the day after his conditioning session.

CONCLUSIONS AND DISCUSSION

The results of this study support the hypothesis of a functional ablation occurring during hypnotic age-regression. Hypnotic age-regression appears to be authentic rather than simulated.

The degree of such ablation is given by our data. It is evident that, while the experimental group reproduced its mean number of acquisition CRs, this reproduction, trial-for-trial, was not precise. There is little doubt that the hypnotic age-regression procedure enhanced the response being measured, but that it was not recalled in exactly the same manner as it had been originally established. However, it does seem that through hypnotic age-regression we have partially by-passed or obliterated the extinction process.

Certainly we would not expect an obliteration of extinction, for in age-regression to childhood we do not erase all previous material, but merely bypass it to some degree. People do return to adulthood from age-regression to childhood! On the other hand, it may be that newly learned responses are, in fact, erased. The determination of this is left to future studies, but regardless of whether we have circumvented or erased the extinction process, it is most certain that words as stimuli-to-action are involved.

Corn-Becker, Welch, and Fisichelli (13) have already demonstrated the conditioning factors in hypnosis. That the ablation theory of hypnotic age-regression embraces a verbal-conditioning theory is clear. The words inducing hypnotic age-regression appear to be the stimuli for the return of the extinguished, involuntary response. The experimental group re-established the extinguished response; the control group did not. In fact, the control group continued to extinguish.

Despite these facts, a question arises: Were the experimental subjects voluntarily responding in order to simulate CRs and thereby to please the experimenter? In most human behaviors in which voluntary and involuntary properties are involved, the voluntary response is virtually indistinguishable from its conditioned counterpart. Since eyeblinks involve striated muscle, they are no exception. There is little doubt that our CRs fall within the latency range of voluntary responses.

However, that the responses given were not voluntary in the sense of being deliberate is supported by two factors: (a) the verbal report of the subjects and (b) the shape of the age-regression curves in Figure 1. When questioned at the completion of the study, none of the subjects were able to state the purpose of the experiment beyond that it had "something to do with hypnosis and that other part [all eyeblink procedures]." If an attitude factor would motivate subjects to attempt to simulate CRs in order to please the experimenter, it follows that they should want to continue to please him with truthful answers. Certainly, here was the place not only to please the experimenter but to boost their own egos with a verbal display of their brilliant deductive powers. None did.

If, however, one still suspects that attitude factors would interfere with the truthfulness of subjects, the shapes of the age-regression curves offer more objective evidence against the assumption of voluntary responding. Although the rise of the age-regression curves over blocks of trials is significantly less than that of the acquisition curve, a rise is evident. If the subjects were voluntarily responding in an attempt to simulate CRs, one would not expect such a rise.

To attribute this acquisition-like acceleration to a desire to please the experimenter assumes vast sophistication on the part of the subjects. It assumes that the subjects knew, or at least suspected, what was expected of them. However, once they were age-regressed, the only instruction was to look at the light before them. No instructions were given concerning responding or not responding, much less the manner of response expected. In fact, at that point the experimenter himself did not know what the appearance of the response curves would be. That the subjects could have been self-instructed to respond is plausible, but that they could have been self-instructed to respond in the manner in which they did assumes that they intuitively knew the general shape of CR acquisition curves, a matter it has taken psychologists years of experimentation to clarify! Voluntary responding seems out of the question.

The fact that the age-regression subjects did not precisely reproduce their

prior behavior immediately raises still another question: Is the role-taking theory of hypnotic age-regression supported by such a finding? Although Young (1), Sarbin (2), and Orne (3) offer such findings in evidence for the role-taking theory, a closer inspection of the theory is necessary.

Sarbin's role-taking hypothesis depends on the subject's motivation, perception of role, and role-taking aptitude. These concepts give the impression of volition, with which we have just dealt, on the part of the subject. One gets the feeling that the subject is supposed to be pretending, going-along-with, and, in general, humoring the hypnotist in accord with varying amounts of the three attributes above. Sarbin's example of role-taking strengthens this opinion: "Compare, for example, the performance of Barrymore as Hamlet with the efforts of a high school senior." (14, p. 261). The age and experiential differences in this example speak for themselves.

Though Sarbin's writing often leads to the conclusion of volitional and conscious processes operating in role-taking, he states in an early paper that stage-acting is conscious, whereas everyday role-taking is unconscious. That role-taking is unconscious does not differentiate it from conditioning. Heferline, Keenan, and Harford (15) have recently conditioned an invisible thumb twitch in subjects completely ignorant of the process. The present concern with motivational research and studies that demonstrate verbal reinforcement in psychotherapy (16) is pertinent here also.

In fact, it is difficult to find a fundamental difference between the role-taking and the verbal-conditioning explanations of hypnotic age-regression. Both views can certainly be guilty of circularity. To say that age-regression is role-taking behavior, and then to account for individuals who do not regress as poor role-takers tests one's gullibility. By the same token, it is no better to account for the same failure to regress as due to the presentation of an incorrect stimulus. Neither theory can claim a monopoly on circular explanations.

Speaking of hypnosis in general, Sarbin states, "A person cannot take a role for which he lacks the necessary response patterns." (17, p. 275). These response patterns are learned early in life and could well be results of a complex conditioning process. With respect to hypnotic age-regression, the concept of "necessary response patterns" would seem to weaken the position of the role-taking theory. Certainly, each individual possesses the response patterns for his age-regression. Each has already lived his past, acquired those patterns, and needs only the impetus to relive them. This impetus comes from the experimenter in the form of words.

If hypnotic age-regression is verbal conditioning, why did the subjects not give the same responses for the same stimulus? Since we are aware that nothing in nature exactly repeats itself, no clear-thinking adherent of the ablation theory would expect an exact duplication of past behavior. As Arnold has pointed out, subjects return to their own past, not necessarily to the past which the experimenter intended (18). Thus, each subject of the present study had an entire set of responses capable of being elicited by the stimuli given. Subjects are not always interested in the same things as experimenters, and the true CR of any study is not necessarily the one measured by the experimenter. This was exemplified in the present study by the experimental subject whose eye teared throughout the regression as it had during conditioning.

According to Sarbin, such behavior could be interpreted as due to differences in the role perceptions of the subjects. The only difference between

this and stating that one stimulus elicits slightly different responses from different individuals is in the placement of the stimulus-to-action. In the role-taking conception the more important stimuli-to-action reside within the subject; he picks and chooses, consciously or unconsciously, the ways in which he will act out the roles named for him by the experimenter. In other words, the extent of hypnotic age-regression depends in a large part on the subject-variables mentioned before.

The verbal conditioning hypothesis implies that the stronger stimuli-to-action are external to the subject, so that the subject's responses to the verbal stimuli from the experimenter are little altered by subject-variables. Just as role-taking does not totally ignore the influence of the experimenter, verbal conditioning cannot deny the importance of subject-variables. We have obvious evidence of their operation in the present study. However, it is the relative prominence of these two variables which is important. To state that conditioned responses are the basic units of human behavior does not reduce us to automata. It merely yields a hypothesis for studying the organizational powers of the human being.

In our own case, a verbal conditioning explanation of hypnotic age-regression appears to be superior to that of role-taking for the purpose of prediction. In verbal conditioning we assume, as does role-taking, that something occurs within the organism between the reception of the stimulus (word) and the eliciting of the response (hypnotic age-regression behavior). However, a verbal-conditioning explanation assumes that this something affects relatively little our prediction of the range of responses elicited by a known stimulus or set of stimuli. If this formulation is, in fact, the case, we should be able to predict this range within the limits of reason. This we have done in the present study.

SUMMARY

The present study was an attempt to evaluate the ablation theory of hypnotic age-regression by utilizing a brief (three to four weeks) regression period, with a previously established involuntary, conditioned response as the criterion of authenticity.

Three to four weeks after the individual establishment of a conditioned eyeblink response in 12 subjects, this response was extinguished. Immediately six of the subjects (the experimental group) were hypnotically age-regressed to their acquisition periods and tested for the return of the conditioned response. The other six subjects were similarly tested but not age-regressed. Thus, they served as a control group.

The two groups were equal with respect to both the acquisition and the extinction of the conditioned response. The results of the testing period show that the experimental group reproduced a close approximation of their acquisition behavior; the control group continued to demonstrate extinction behavior.

From the results we conclude:

1. The ablation theory of hypnotic age-regression is substantiated. Hypnotic age-regression appears to be authentic rather than simulated.

2. Although the operation of some subject variables is evident in the fact that the experimental group did not precisely reproduce their acquisition behavior, words spoken by the experimenter seem to take precedence. The major stimuli-to-action in hypnotic age-regression as studied here appear to lie outside of the subject. Thus, a verbal-conditioning theory of hypnotic age-regression offers more direct predictability than a role-taking theory.

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HYPNOTIC AGE-REGRESSION: A REVIEW[1]

by J. W. Gebhard, Ph.D.[2]

If a normal subject is asked to search for a specific old memory, say, the day of the week on September 16, 1931, he typically gives up before half trying, for such a request seems unreasonable. In hypnosis, however, there exists a procedure for recovering old memories that utilizes high motivation and specificity in the instructions to the subject and seeks thereby to increase the accuracy of the response. To achieve this it is emphatically suggested to the hypnotized subject that he is actually living on September 16, 1931, and that his age is now what it was at that time. The ability of this method to recover some old memories in a spectacular way is generally admitted, but its power for dating memories with precision is greatly debated. The claims range from those purporting recall as far back as the time of birth and the instant of conception (32, 39), to those asserting no superiority whatever of hypnosis over the waking state.

The question at issue is how memories are stored in the neural tissues. If they are stored as complete systems in unaltered state, hypnotic dissociation may be a key that unlocks the files. If memories progressively change and decay with time, neither hypnosis nor anything else will reinstate what no longer exists. The purpose of this review is to assess the experimental data that have been collected on recovering remote memories by hypnosis in order to form an estimate about the age and condition of the memory material that may exist in the neural store of an adult.

HYPNOTIC RECOVERY OF MEMORY

The reversion of the individual at one stage of development to forms of behavior normally occurring at an earlier stage of development is known as regression. This process may be characterized by the reactivation of specific old habits, or the primitivation of action, or both (96). Of the several ways in which regression may occur, we are concerned here only with the use of hypnosis to produce experimentally a controlled return to a suggested prior age. This is called hypnotic *age-regression*, and the dramatic changes in the adult personality that often take place were reported by Krafft-Ebing (52) in experiments on a single patient done in 1887 and by Binet and Féré (5), who cited the observations made by both Richet and Beaunis on enhanced recall. Spontaneous regressions under hypnosis, even under self-hypnosis, have also been described and have a bearing on the controlled regressions (12, 29, 38, 92). When a subject is hypnotically carried into the future to an age later than his current age, the result has been called by Kline *age-progression* (41). This kind of experiment was first reported by Dolin (14) and later by others (36, 41, 44, 47, 49, 60, 84). It is clear that the recovery of old memories can be studied only in age-regression. Age-progression is of interest in this review only as it applies to the question of role-

[1] This report was prepared under Contract NOrd 7386 between the U. S. Navy, Bureau of Naval Weapons, and The Johns Hopkins University. The invaluable assistance of B. W. Kuvshinoff and R. P. Illwitzer in the Russian Translation Group of the Applied Physics Laboratory is gratefully acknowledged. I also wish to thank Richard Cutter, National Library of Medicine, Peter Petcoff, Slavonic Section of the Library of Congress, and the many kind librarians at the National Institutes of Health for their aid in tracking down the obscure references.

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playing, a possible artifact that has always made the interpretation of experiments in hypnosis difficult.

Method

In controlled age-regression the subject is first deeply hypnotized and the experimenter suggests, with suitable emphasis, a chronologically earlier age. The term "deep" introduces an initial difficulty. There is some agreement that hypnosis must be sufficiently deep to produce convincing age-regression (88), but satisfactory criteria for the objective measurement of hypnotic depth are not always used. Careful experimenters make an effort to specify depth by the use of such scales as those proposed by Friedlander and Sarbin (86), and Davis and Husband (11). Even with the aid of a scale, however, the depth of hypnosis is a matter of the experimenter's judgment.[3]

During the hypnotic state the responses elicited from the subject, or emitted spontaneously by him, are observed and recorded by the experimenter. As criteria for the validity of hypnotic age-regression, the least satisfactory of these have been reported by nearly all observers: infantile or childlike behavior such as weeping, thumbsucking, baby talk, feeding from a bottle, bedwetting, and the like. The most objective criterion that has been applied is the attempt to recover physiological mechanisms that differ clearly in the adult and immature individual. These mechanisms are reflexes, the electroencephalogram (EEG), and the heart rate. Next in objective rigor are psychological measures capable of being scaled, such as intelligence and drawing tests. Finally, use has been made of projection tests like the Rorschach that measure broad aspects of the personality.

A Basic Problem

In considering the validity of the recovery of old memories by hypnosis, three kinds of possible behavior must be examined. Of greatest interest for the present purpose is the possibility that the subject may be responding exclusively on the basis of neural mechanisms that were established in the nervous system prior to the cut-off date suggested by the experimenter. Of little interest is the second possibility that he may be using memories of any age to act out a role based on his interpretation of the problem posed to him by the experimenter. Finally, his responses may be a combination of both of these in varying amounts. It has been very difficult to produce unobjectionable evidence for the first behavior.

Erickson and Kubie (19) noted the first two aspects of this problem and in a laudable effort to distinguish them introduced the term *revivification* to designate the process of reliving under hypnosis "the patterns of behavior of the suggested earlier period of life in terms only of what actually belonged there. It is not a 'regression' through the use of current memories, recollections, or reconstructions of a bygone day. The present itself and all subsequent life and experience are as though they were blotted out." By *regression* Erickson and Kubie meant the recall and acting out of past events, with current memories being available.

Weitzenhoffer (108, 110), deploring the indiscriminate use of the term "regression" to cover all kinds of observations, pointed out the third, or mixed, aspect of the problem. He proposed the term *regression type I* for the role-playing kind of response, *regression type II* for a true return to a past psychophysiological state, and *regression type III* for the mixed con-

[3] For a discussion of scaling hypnotic depth see Weitzenhoffer (109). Solovey de Milechnin (98) has an interesting account of what depth may mean in hypnosis.

dition. The term *revivification* was retained for "the process by means of which such a psychophysical shift is brought about in association with a temporary ablation of certain 'future' events . . . . " (108). In Weitzenhoffer's opinion the literature prior to 1953 contained no reports of type II. All instances were of the mixed type III.

In the following sections Weitzenhoffer's classification will be applied wherever possible. While the emphasis will be on the work using objective criteria, some reports of clinical observations are also included.

Physiological Studies on Old Neural Mechanisms

The physiological alterations producible by hypnosis have been recently reviewed, and the validity of many of them is now well established (9). Our concern here, however, is with the use of physiological criteria for testing the validity of age-regression.

Electroencephalogram. Kupper (53) was the first to report distinguishing between normal and abnormal EEGs existing in the same subject's life at different times by hypnotic age-regression. A 24-year-old patient under study for convulsive seizures was found to have an abnormal EEG in the waking state that had existed since age 18. Regressed to age 12, the EEG was found normal. Serial EEGs remained normal in intervening years of regression, becoming abnormal abruptly in the 18th year. The change-over point was found to coincide with an episode at age 18 involving great anxiety. The seizure could be activated hypnotically by recalling the emotionally critical parts of this episode.

Schwarz, Bickford, and Rasmussen (94) verified the possibility of reactivating seizures by hypnosis. They used 10 patients suffering from convulsive disorders without known organic cause. Two patients had moderate dysrhythmias in the EEG, the others had either minimal dysrhythmias or were normal. In no case, however, was the induced seizure accompanied by a change in the EEG. The possibility of direct comparison with Kupper was lost, since none of the patients with abnormal EEGs was regressed to an age prior to the first onset of seizure. Another study was done on 16 patients whose seizures had probable organic cause. Eleven of these cases showed "moderate" to "marked" dysrhythmias in the EEG. In no member of the organic group was it possible to initiate seizures by hypnotic suggestion. It is reported that "in many cases regression was carried to the time of the patient's first or last seizure . . . . " without change in the EEG. Unfortunately, again, there is no indication of what might have happened to the EEG had regression been carried to an age earlier than that of the first seizure.

Ford and Yeager (22), however, reported on a 25-year-old patient with a history of grand mal seizures dating from a head injury at 16 months. One year before a craniotomy his EEG consisted of slow 4-6 cps waves with occasional random spikes in the right frontal area. After "a resection of a porencephalic cyst of the right frontal lobe" some seizures still occurred, but the EEGs were normal on two occasions and abnormal on two others. A year later he was regressed to the year before the operation when his EEG consisted of slow waves and a few spikes. The EEGs now showed "mild cerebral dysrhythmia with no focalization and were essentially the same as those taken during the waking state."

Schwarz et al. (94) further reported that one subject showed an altered EEG when regressed to age three. A frequency analysis of the EEG pattern showed a shift from normal alpha predominance to the delta and theta frequency bands. This result, however, was neither constant nor reproducible.

Five other subjects when in "deep regression" showed no notable EEG change although infantile behavior was grossly manifest. True and Stephenson (106) studied six subjects regressed as far as an age of one month and found no changes in the adult EEG. McCranie et al. (58) similarly regressed 10 subjects to one month with no effect on the adult EEG.

Tables 1 and 2 summarize the results of the investigations to date. The conclusion to be reached from Table 1 on convulsive patients is that Schwarz et al. (94), who had an adequate number of cases, stopped their work too soon. The situation is better in Table 2, where it appears clear that the neural processes underlying the immature EEG cannot be reconstituted in normal adults by hypnotic age regression.

Unconditioned Reflex. As early as 1928 Gakkebush alluded to work that he and Polinkovskii had already completed on simple unconditioned motor reflexes observed under hypnotic age regression (24, 25). When the complete account appeared, this was restricted to the sucking reflex reported in detail on one case, a male of 34 years, although other subjects were

TABLE 1

THE EFFECT OF HYPNOTIC AGE REGRESSION ON THE EEG OF PATIENTS WITH CONVULSIVE DISORDERS

Investigator	No. of Cases	Diagnosis	Waking EEG	EEG when hypnotically regressed	
				To time of first seizure	To time before first seizure
Kupper (53)	1	Not organic	Abnormal	Abnormal	Normal
Schwarz et al. (94)	10	Not organic	Abnormal except for 2 cases	Unchanged	Not done
Schwarz et al. (94)	16	Organic	Abnormal	Unchanged	Not done
Ford & Yeager (22)	1	Organic	Abnormal	Abnormal	Abnormal

TABLE 2

THE EFFECT OF HYPNOTIC AGE REGRESSION ON THE EEG OF NORMAL ADULT SUBJECTS

Investigator	No. of Cases	Waking EEG	Age to which regressed	Regressed EEG
Schwarz et al. (94)	1	Adult	3 yrs.	Infant, but not constant
Schwarz et al. (94)	5	Assumed adult	Not stated	Unchanged
True & Stephenson (106)	6	Adult	1 mo.	Adult
McCranie et al. (58)	10	Adult	1 mo.	Adult

tested (26).[4] During a session of 55 min. on one day the hypnotized subject was told he was just born, and a nipple was placed in his mouth. He sucked weakly. When told he was wet and cold there was no reaction. It was suggested during succeeding tests that he was 2 weeks, 1, 2, 4, 5, 6, and 9 months old. The sucking response became energetic and greedy. Interposed tests up to nine months involving higher levels of response, such as "Mama is not at home" and "What is your name?" produced no reaction. At nine months "Mama" was successfully repeated, and crying was elicited when he was told that he was alone and afraid. A week later the ages of 9, 11, 12, 13, and 14 months were suggested during a half-hour session. Sucking was present at 11 months, fell off at one year, and disappeared between 12 and 13 months.

The development of conditioned motor associations was followed under hypnosis from one month of age to 28 months (26). Starting with no reactions, grasping, holding up the head, sitting, standing, and walking were observed to develop along a pattern that progressed with suggested age. "We saw how the 'infant' progressed from mass, diffused, uncoordinated motions to the precise, strictly differentiated functioning of separate muscular groups."

N. D. C. Lewis produced a dorsiflexor response to plantar stimulation by hypnotic age regression about 1926 (28). This led to the much-quoted study of Gidro-Frank and Bowersbuch (28), who reproduced this phenomenon in three subjects, one aged 42 and two aged 21. While the general infantile behavior of the subjects differed somewhat, all three gave normal plantar flexion responses down to a regressed age of six months. At about five months and below, however, the normal reaction changed to dorsiflexion of the great toe with fanning of the outer toes.

This startling observation was first confirmed by True and Stephenson (106), who examined five subjects giving plantar flexion during the waking state and one subject who gave an equivocal Babinski sign. All except the equivocal case showed dorsiflexion of the great toe when regressed to one month by hypnosis. Since the positive Babinski sign sometimes occurs during sleep, care was taken to assure that the subjects had not gone to sleep during the experiment. The writers further noted that two of their six subjects exhibited the sucking reflex at one month and that a third showed the Moro reflex to the sound of an inadvertent cough.

[4] Bibliographic use of this reference by students contributing to the literature of hypnosis must be viewed with amazement. For nearly 30 years it has been cited by numerous writers always as: Hakebush, —, Blinkowski, —, and Foundillere, R. The reference is apparently due to an abstract submitted by Willoughby (114). However, the staff of the *Psychological Abstracts* was able to locate in its old files only an abstract in French from an unidentified source. This suggested the *Année Psychologique*. Sure enough, an abstract by Mazé (61) was there with a slightly different spelling of the names, but still with the initials missing. Willoughby's abstract was an abbreviated translation of this.

An attempt to locate anywhere in this country a library with holdings of *Trudy Instituta Psikhonevrologiia, Kief*, was unsuccessful. A copy of this journal was finally obtained through courtesy of Mme. S. A. Zerchaninova, V. I. Lenin State Library of the USSR in Moscow. The missing initials of Gakkebush, et al. (26) were conspicuously present, and Blinkowski turned out to be Polinkovskii! Since the pages of the Moscow copy were uncut, one may conclude that fewer have seen this work than have quoted it. Inasmuch as it has been used so frequently to support the position of *bona fide* age regression, the findings of Gakkebush et al. will be treated at some length in various sections of this review. The complete protocols in the original are very interesting.

McCranie et al. (58) also reported sucking responses at five months and below. In tests of the plantar reflex at this regressed age three of their 10 subjects gave the Babinski toe response. This returned to normal plantar flexion when they recovered their usual chronological ages.

Finally, several brief confirming observations have been given without details on the production of infantile reflexive behavior (54, 90, 108). LeCron states that he "confirmed this (dorsiflexion) finding with several subjects" (54). At the same time it was determined that the sucking reflex was present in these subjects when regressed to infancy. Sarbin noted in a footnote to a discussion of this problem that he had seen the positive Babinski only once. The response appeared spontaneously in a subject in deep hypnosis. Weitzenhoffer's comment was also in a footnote.

Conditioned Reflex. Gakkebush et al. (26) studied the development of a conditioned reflex under age-regression. At the suggested age of nine months the subject was shown a burning match. He grasped the flame and was burned. On tests 10, 15, and 18 minutes later the same response occurred. On the fifth trial the flame was refused and was called "ziza," baby talk for "spichka" (match). The match was again avoided on the sixth trial three minutes later. Two minutes after this the age of 12 months was suggested, and the match was approached but not grasped. This resulted in a slight burn. Five minutes later the match was again refused. Conditioning to a pain stimulus was held to have occurred on the fourth trial. It is unfortunate for the questions asked in this review that the experimenters did not then suggest an earlier age, say six months, and repeat the test.

More germane to the problem is the effect of regression on acquired responses shown in the work of LeCron (54, 55), McCranie and Crasilneck (57), and Edmonston (17). In the first two studies the unconditioned responses used were hand withdrawal to an electric shock, and eyelid closure to a mechanical stimulus. LeCron tested two subjects each on the hand jerk and eye wink and found that the conditioned responses to a conditioned auditory (buzzer) stimulus disappeared when the subject was regressed hypnotically to an earlier age.

McCranie and Crasilneck used six subjects in each experiment. They found that while the hand withdrawal conditioned to an auditory stimulus disappeared under regression, the eyelid reflex conditioned to a tap on the wrist was retained at any age level. McCranie and Crasilneck did not directly account for the discrepancy between their results and those of LeCron. They distinguished between conditioned responses of voluntary and involuntary movements and held the involuntary to be unaffected by age regression, whereas the voluntary can be influenced by psychological processes. In suggesting that their subjects "somehow 'knew' what was expected of them under hypnotic age regression and, in the case of a voluntary movement, were able to comply with that expectation," they imply that this may have been true of LeCron's subjects. This does not explain, however, how LeCron's subjects were able to lose the involuntary response as well as the voluntary one.

Edmonston (17) conditioned the reflex produced by a puff of air on the cornea to the simultaneous presentation of a click and an increase in brightness. Experimental and control groups of six subjects each were matched for age and sex. The eye wink was electrically recorded and the conditioning trials were associated with the approximate time and date they were given. Three or four weeks later the conditioned response was abolished

for both groups by enough extinction trials to meet a criterion. The experimental group was then regressed hypnotically to the time and date of the original conditioning and tested by a further series of 80 extinction trials. The hypnotic subjects were then advanced to the current date, given amnesia for the first 80 trials, and the procedure was repeated exactly. The test for the control group also consisted of two sets of 80 extinction trials given immediately after the series necessary to extinguish the response to the criterion level. The results showed that (a) the experimental and control groups did not differ significantly in either the initial acquisition or extinction of the response, (b) the experimental group produced significantly more responses than the control group during both test runs of 80 trials each, and (c) while the hypnotic subjects did not differ in the mean number of conditioned responses produced during either the acquisition or the two test series, their response rate was lower during the test trials.

Edmonston's study on conditioned response behavior in age-regression is the best to date and his conclusion is that the functional ablation rather than the role-playing theory of regression is upheld. He did not, however, require his non-hypnotized subjects to attempt to place themselves in time at the beginning of their acquisition trials. Young and Orne, I believe, would have considered this crucial for the comparison he made.

Other Physiological Effects. Many years ago Erickson (18) reported a case wherein a 19-year-old patient, regressed to age 17, lost consciousness when reliving under hypnosis a homicidal assault suffered at that time. This kind of phenomenon has also been observed by Weitzenhoffer (108).

Ford and Yeager (22) cited a case in which a right homonymous hemianopsia existed in a patient prior to an operation to remove a colloid cyst from the floor of the third ventricle. After the craniotomy vision returned to normal. When the subject was regressed to a time before the operation, however, the hemianopsia was found to return.

LeCron (55) reported the unpublished experiment of W. H. Roberts and D. Black on a single case of a refractive defect existing in a subject since childhood. Glasses were first fitted at age 12. The results of an examination of vision during the waking state and during hypnotic regression to age 7 showed "a small but measurable improvement of both near and far vision . . . . " when under hypnosis. Direct suggestions for improvement under unregressed hypnosis gave no improvement.

Another unpublished study reported by LeCron (55) is due to J. G. Watkins and B. J. Showalter. The eye movements of a 19-year-old subject were photographed during reading at the regressed age of 6 years, 10 months. The ophthalmographic record at this age showed the poor ocular coordination and stability typical of a first grader.

True and Stephenson (106) attempted to recover the higher pulse rate of infancy by regression. In four of six subjects the pulse rate rose with the induction of hypnosis, in one case markedly so although the return to the pre-induction level was rapid. With regression to eight years, six years, and one month, there appeared to be a slight elevation in pulse rate over the pre-induction level, but the authors reported great variability and drew no conclusions from their limited sample. Only in one case was there a clear-cut increase.

Psychological Studies on Old Neural Mechanisms

Recovery of Recently Learned Material. The first problem considered is

the recall of newly acquired material under hypnosis without explicit instructions for regression. Regression appears to be implicit, however, in that the subject is required to reproduce material that was learned earlier, even though earlier means in the immediate past. It should be made clear that we are concerned here only with the problem of learning the material in the normal waking state with recall under hypnosis, and not the reverse.[5]

Young (114, 115) found in a sizeable experiment that hypnosis improved the ability to recall early memories but had no effect on those recently acquired. His material for the latter consisted of nonsense syllables, word pairs, lists of words in logical order, and objects normally displayed in a room. However, I must agree with Weitzenhoffer (109) that it is not clear that the original learning was accomplished in the waking state and that the comparison was made between waking and hypnotic recall for material so learned. Nevertheless, I assume, like Weitzenhoffer, that this was the case. While Young based his conclusion on the variability of his subjects, there is a suggestion in his data that, whereas his average hypnotized subjects performed no better than the control group, the more deeply hypnotized somnambulistic subjects recalled more than the controls.

Huse (35) used only nonsense syllables. From results obtained on eight subjects with a 24 hour delay between learning and recall she concluded that there was no hypermnesia under hypnosis.

Similar material in the form of lists of three-place numbers was used by Mitchell (63) to examine the effect of the trance state on recall. Two subjects were used with a delay between the learning and relearning conditions of only a few minutes. The results measured by savings in trials, savings in errors, and recall again showed no improvement under hypnosis as compared with the waking state.

Stalnaker and Riddle (100) studied the same problem but tested for the memory of meaningful material, such as poetry. Another difference was that the material had been memorized a year or more prior to the experiment. Twelve subjects were examined on the same material in the waking and hypnotic conditions. The results now showed about 54 percent more recall under hypnosis, giving clear evidence for hypermnesia.

White et al. (112) introduced their experimental work with an analysis of the earlier studies, in which they pointed out that, while hypnotic hypermnesia had been shown to occur for remote but not recent learning, it was also true that it occurred for meaningful rather than nonsense material. They chose, therefore, paired nonsense syllables like those of Huse, and poetry like that of Stalnaker and Riddle. To this they added short motion picture sequences to test the effect of nonverbal material more like the kind often reported in the clinical literature. Eight subjects were used for the carefully memorized material, serving both as control and experimental groups. For the casually learned material in the films, five new experimental subjects and four controls were used. Tests for the memorized material were conducted 24 hours after learning; the film material was tested from one to five days after viewing.

The results showed that different kinds of material produced different amounts of recall under hypnosis. When compared to normal recall, the gain due to hypnosis was —15.7 percent for nonsense syllables, 53.1 percent for poetry, and 83.1 percent for the scenes. The results for nonsense pairs agree

[5] For a review and estimate of the problem of the efficiency of initial learning in the hypnotized condition, see Uhr (107).

with Young and Huse, and those for poetry agree with the finding of Stalnaker and Riddle. The scenic material, tested here for the first time, responded best of all to recall under hypnosis.

Rosenthal (83) suspected that hypnotic hypermnesia might better exist for emotionally charged material. Emotional content could much more readily reside in meaningful material than in nonsense lists. Using 13 subjects, Rosenthal tested this hypothesis in a series of well-controlled experiments with nonsense syllables, poetry, and profane and obscene words. Emotional tension was introduced by success-failure instructions, emphasizing special importance to the subjects of obtaining good recall scores. The results showed that hypermnesia was possible even with nonsense syllables when the items were emotionally stressed. The findings for unstressed nonsense material and poetry agreed with the work of others.

The 24 subjects of A. B. Sears (95) were asked to remember miscellaneous objects arranged on a table. The experimental design was selected to test only the hypothesis that hypnosis would have no effect on either immediate or delayed recall. For the experiment as a whole there was consistently better recall under hypnosis than in the waking state. While Sears properly did not make a quantitative statement of the improvement he found, if one is permitted to attempt a few estimates from his data, it may be as much as 25 percent in some comparisons.

Finally there are the memory experiments of Eysenck (20), based mainly on one subject. Here I again agree with Weitzenhoffer (109) that the procedure followed is unclear. I incline to the view, however, that all of Eysenck's tests were made either in the waking state or in the hypnotic state, and that he did not compare hypnotic and waking recall for material learned originally in the waking condition. For example, it would be difficult but perhaps not impossible to measure immediate memory span by presenting the digit series in the waking state and testing for recall in the hypnotized state. His finding that there was no improvement under hypnosis for tests of memory, therefore, should not weigh greatly in this review.

Mental Age in Regression Measured by Intelligence Tests. An air of excitement clung for a time about the possibility of objectively measuring the mental age (MA) of a subject regressed hypnotically to a suggested chronological age (CA) by standard intelligence tests. Russian investigators first reported success in doing this. Gakkebush et al. (26) and Platonov and Prikhodivny (80) used the Binet-Simon test to assess the MAs of subjects regressed to various age levels. Three years later the results of the second investigators were again reported in English translation (81).

The one subject of Gakkebush et al. was carefully tested at suggested CAs from three through 11 years. Mental ages were found from five to 11 years as shown in Table 3, where it is noted that from the age of eight the subject's MA began to lead his actual age.[6] Data from the Rorschach and an interview provide some insight into personality disturbances that may account for this.

In Platonow's study (81) the MAs obtained for two cases showed a rough correspondence with the suggested CAs of 4, 6, and 10 years, although they tended to be somewhat higher. The third subject produced MAs that were markedly higher at all regressed ages. Platonow diagnosed him as of "premature development," supporting this

[6] I am indebted to Dr. Nancy Bayley, National Institutes of Health, for examining the published records. She reports the uneven mental development shown in these tests to be characteristic of many children.

TABLE 3

BINET-SIMON MENTAL AGES AT DIFFERENT CHRONOLOGICAL AGES SUGGESTED UNDER HYPNOSIS. THE SUBJECT'S ACTUAL AGE WAS 34. After Gakkebush et al. (26)

CA	3	4	5	6	7	8	9	10	11
MA	—	—	4.7	5.8	7.2	9.8	12.2	13.3	14.3

by the comment that he was then "one of the brightest students of a technical school". In no case, however, was evidence presented on the measured IQs of these subjects at any age in the waking state. The closest to this was an attempt to have the subjects act out the taking of test items at regressed ages in the waking state. In this they were said to have failed.

Young (116, 117) proceeded shortly to examine the Russian work. He used 14 experimental subjects and seven controls who could not be hypnotized. These subjects in the waking state had IQs ranging from 96 to 110. Items from the Stanford-Binet test (1916 edition) given to a group of 10 subjects regressed to age three gave an average MA of about 4 yrs. 8 mos. Individual tests on nine subjects, five of them the same, gave an average MA of about 5 yrs. 11 mos. The seven control subjects simulated three-year-old behavior to achieve an average MA of 5 yrs. 5 mos. Young's subjects, therefore, showed the same tendency toward MAs higher than the regressed CAs as had Platonow's. He suggested that performance on intelligence tests under hypnosis was role-playing and in his review of the literature held the view that there had been "no report of a rigorously done research favorable to age regression" (118).

One point of hypnotic technique and several points of rigor in Young's experiments have been questioned. Pattie (69) referred to Erickson's objection that Young had regressed his subjects too quickly to an insufficient depth. Dorcus et al. (15) criticized his preliminary report for inadequate controls and claimed that "almost any conclusion could be derived from this investigation." It is not apparent from Young's abstract of the paper given before the American Psychological Association in 1937 that he drew any conclusion. He merely asked this question when his subjects failed to reach earlier than the six year level when regressed to three years: "Can this mean that the personality as the adult subject experiences it in himself takes form around six years of age?" (116). More cogent points about Young's (117) complete paper were raised by Sarbin. First, "no objective data were available against which to check the validity of the responses on the Stanford-Binet test items," and second, "no systematic attempt was made to consider the effect of variations in the depth of hypnosis" (88). To correct this, Sarbin found 12 subjects on whom MAs had been obtained at CAs of eight or nine and used an index of hypnotizability based on the Friedlander-Sarbin scale (23). MAs were obtained under both hypnotic and simulated regression to the ages of eight or nine. The data are shown in Table 4. In agreement with Young, the MA under hypnotic regression was higher than it should have been had the subject really gone back to the suggested CA. From this Sarbin concluded, "There is no authentic and complete regression to earlier age-roles insofar as intelligence tests are concerned." However, the data also showed that the regression index correlated with the hypnotizability scale, giving rho = .76 for

TABLE 4

STANFORD-BINET MENTAL AGES ON ORIGINAL TEST AND UNDER HYPNOTIC AND SIMULATED REGRESSION

After Sarbin (88)

Subject	MA on Original Test Yrs.	Mos.	MA under Hypnosis Yrs.	Mos.	MA under Simulated Regression Yrs.	Mos.	Regression[1] Index	Hypnotizability[2] Scale
Rc	10	5	12	0	16	9	46	25
Ra	9	2	13	2	16	3	33	23
Ru	10	8	14	3	16	7	21	21
Gr	11	9	15	0	17	2	19	20
Hi	11	11	16	6	17	9	10	24
Ho	14	3	17	2	18	9	10	21
Ea	9	4	15	10	16	5	6	10
De	10	5	15	0	15	2	3	9
Lo	10	0	12	7	12	8	1	17
Br	10	3			14	2	(0)	0
Sa	11	9			18	5	(0)	0
Fi	10	5			19	0	(0)	0

[1] The regression index is taken as $\frac{\text{MA simulated regression}}{\text{MA original test}} - \frac{\text{MA hypnotic regression}}{\text{MA original test}} \times 100$.

[2] Friedlander-Sarbin (23). Zero means not hypnotizable.

nine subjects and rho = .91 if the three nonhypnotizable subjects were included. Against Young's finding that his non-hypnotizable subjects simulated regressed ages as accurately as his hypnotized subjects, Sarbin's data showed much poorer results under instructions to simulate.

On the other hand, positive findings relating hypnotically regressed performance to maturational power appropriate to the suggested CA were reported by Kline (40). Ten subjects were used, the depth of hypnosis was not scaled, and the Otis, rather than the Stanford-Binet test, was given. Different forms of the test were used in the waking state and at the ages suggested under hypnosis of 8, 10, and 15 years. Since raw scores changed with regression while the IQ remained constant, Kline concluded that there was a loss in achievement power consistent with maturational level but no loss in innate brightness.

Over the years there have been many scattered studies based on single cases and open to the criticisms of Young (117), Sarbin (88), and Dorcus et al. (15). Some of these are interesting. Keir (38) reported a case of a 17-year-old hysterical woman hospitalized because of sleepwalking, during which stereotyped behavior patterns occurred. Under hypnosis she spontaneously became a schoolgirl of 13 years at a specific date in the past. In the adult state she had an MA of 16 yrs. 5 mos. and an IQ of 109. At the regressed age of 13 her MA was 14 yrs. 6 mos. The IQ calculated for a CA of 13 years gave an IQ of 106, very close to her normal IQ.

Kline (41, 42, 48) has reported the results obtained with the Wechsler-Bellevue Intelligence Scale Form I given to a 22-year-old woman regressed to age eight. While the IQs computed from the Verbal, Performance, and Full Scale weighted scores were seven to nine points higher under regression than in the normal state, they were probably within the limits of experimental error. Kline conclud-

ed, therefore, that his results were favorable to the validity of age regression.

Spiegel et al. (99) were concerned primarily with what they call an hypnotic technique for "blocking out or ablation of all personality developments from the present back to the specified birthday, thus releasing authentic personality-intelligence manifestations of the appropriate age level." This is simply age-regression, and the evidence they offered for the success of this was Binet test data on one case of an hysterically ill patient of 23 years. Successive tests traced the increase of the MA from 1 yr. 6 mos. to 20 yrs. for 12 regressed ages. There was considerable variation in the IQ, which ranged from 95 to 134. The waking IQ was 123. The discrepancies at certain age-levels were accounted for by the test itself and certain factors of clinical interest in the developmental history of the subject. An attempt to have the subject simulate earlier age-levels was unsuccessful. The authors held for the authenticity of behavior under regression and promised more complete details, but no further report has appeared.

Leeds (56) required a normal subject of 40 years to define words taken from the Stanford-Binet. The development of the ability to define more difficult words was followed from the regressed ages of two to 12 years with the conclusion that the maturation of the intellect was followed.

Performance in Regression Measured by a Test Battery. In a very extensive experiment by Reiff and Scheerer (82), eight perceptual, cognitive, and emotional tasks were designed to evaluate childhood functioning. These performance tests included Piaget's Hollow Tube and Right-Left tests, an arithmetic and word association test, as well as some situations involving childhood memories and responses. Five experimental subjects were tested at the regressed ages of four, seven, and 10 years. Fifteen control subjects were divided into three groups of five. Each group simulated one of the age levels. The detailed treatment of the results of many experiments is summarized in the finding that the hypnotically regressed subjects showed performance consistent with the experimental age level in 51 cases, above it in two cases, and below it in six. The control subjects, on the other hand, simulated the suggested age level in 26 cases, were above it in 33 cases, and below it in one. Reiff and Scheerer's conclusions are quoted as follows:

1. The regressed subjects tended to function at a level consistent with the experimental age.
2. On the various tasks at each experimental age level the regressed subjects functioned more consistently than the simulating subjects.
3. When the regressed subjects deviated from the experimental age, they tended to function below that age level.
4. The simulating subjects tended to function above the experimental age level.
5. The lower the experimental age, the more the simulating subjects tended to function above that level.

Rorschach Studies. Considerable effort has been made to assess personality changes occurring during hypnotic age regression by projective test techniques. First of all are the Rorschach studies ranging from reports on single cases, some hospitalized for personality disorders, to experiments using several normal subjects. While the Rorschach test has the presumed advantage of being difficult of willful manipulation on the part of the testee, it is not well suited for providing quantitative esti-

mates of the age of the memory material on which the responses are based. It does, however, give useful information about the personality structure at different regressed age-levels that may be related to clinical or quasi-normative data.

The first study employing the Rorschach was that of Gakkebush et al. (26) on the 34-year-old subject referred to above. Starting with a suggested age of nine, personality development was traced through the 20th year. The results, when compared with what was known of the subject's background from other sources, were considered to reflect adequately appropriate changes at crucial age levels.

Bergman et al. (3) regressed a 20-year-old soldier suffering from conversion hysteria to eight alternate years starting at age three. In administering the Rorschach test it was stated that "rigorous experimental standards were not used." Nevertheless, repeated testing at several age levels was reported to give consistent results. When the Rorschach responses were compared with case history and clinical findings on the patient, the authors concluded that the "Rorschach findings . . . . at the various regressed age levels . . . . followed closely the clinical data."

A case of a 26-year-old alcoholic was reported by Mercer and Gibson (62) in which the Rorschach was given at the regressed ages of 6, 10, and 14 years. The results were judged to reflect changes in personality consistent with what would normally have been produced on the test at the regressed levels.

Sarbin and Farberow (91) have taken the theoretical position that hypnosis may be regarded "as a form of a more general kind of social psychological activity known as role-taking." This is an extension of a point of view expressed elsewhere (86, 87, 89). As applied to hypnotic age-regression, the subject is held to enact an age-role that depends upon his current concept of his role as an adult, and that of his role as a child of a given age. These must be congruent. This interpretation was examined by the Rorschach records from two adults at the regressed ages of 3, 6, and 13 years. Data from one subject included age 18, and this subject also simulated in the waking state performance at age six. The authors concluded that success in role enactment at regressed age levels was consistent with the adult personality structure of the subject.

Norgarb's (65) single case was that of a normal personality in a 20-year-old man. The Rorschach was given in the waking 20-year-old state, at 20 years under hypnosis after total amnesia for the previous test was suggested, and at the hypnotically regressed ages of 5, 8, 11, 14, and 17 years. The complete protocols and scoring are given in the paper. The records were interpreted clinically and normatively, and the conclusion was reached that the subject's development during 15 years was adequately traced by the test. In Keir's (38) case of hysteria, on the other hand, a Rorschach reader unfamiliar with the history of the patient found the personality unchanged by hypnotic regression to an earlier age.

The most extensive study with the Rorschach is that of Orne (66), who used 10 subjects from 17 to 26 years of age. These were regressed to age six, and the test was given in both the waking and hypnotic states. Orne found incomplete evidence of true or complete regression, and concluded that the personality of his subjects remained adult even at the suggested age of six.

Thematic Apperception Test. In several papers, Kline has studied the usefulness of hypnosis for investigating the origin of occupational interests (45,

51). Most recently, Kline and Haggerty (50) showed four cards from the TAT to a 29-year-old subject in both the hypnotic and waking states. Hypnotic regression was to the ages of 7, 10, 13, and 19. These conditions were alternated with waking simulation of the same ages. The interesting protocols are given complete, and the language used by the subject was analyzed by word count and the percent usage of the parts of speech. Waking simulation revealed predominantly adult behavior, in which the total response to adult pictures was higher than the response to the pictures more suitable for children. Also, the percentages of word usage did not change with simulated regression. Hypnotic regression, on the other hand, showed both higher verbal productivity on the lower age level cards than on adult cards, and a progressive change in word usage with increasing regressed age. Kline and Haggerty found confirmation in these findings of the "neuropsychological validity of hypnotic age regression."

Drawing Tests. Gakkebush (25) analyzed drawings made under hypnotic regression and found them to correspond in detail with samples obtained from the subjects' relatives. Dolin (14), however, was less convinced by the childlike productions he found in drawing and clay modeling.

Bergman et al. (3) included in their paper reproductions of nine Goodenough drawings (30) obtained from their subject: one each at eight regressed ages and one at age 20 in the waking state. Since the instructions for giving the test were not followed, the productions contained many heads and busts. Therefore, it is impossible to score the drawings for age except very roughly. At higher suggested CAs they appear to reflect an increase in MA to an extent, and the authors interpreted them clinically as substantiating the Rorschach records and the subject's developmental history. It is curious to note that Leeds (56) also obtained incomplete drawings showing largely the upper part of the body. Leeds' subject claimed that drawing tended to destroy the relaxed state of the trance, and he stopped working on the pictures before he came out of it.

Mercer and Gibson's alcoholic patient at a regressed age of six had an MA of 5 yrs. 3 mos.; at 10, the MA was 10 yrs. 6 mos.; at 14, 12 yrs. 6 mos. (62).

Another study conducted on one subject is that of Kline and Guze (49), who used the 23-year-old woman reported upon in other work on regression (41, 43). The H-T-P task, which is to draw a house, a tree, and a person, was given in (a) the normal waking state, (b) the waking state simulating age six, (c) the hypnotic state simulating age six, and (d) the hypnotic state with regression to age six. The resulting pictures were analyzed clinically and were reported to "reflect changes in the neuropsychological organization of the individual with particular emphasis upon the perceptual and learning mechanisms." The drawings are reproduced in the report. The first three are much the same to an unpracticed eye, but the pictures drawn at the hypnotically regressed age of six are obviously more primitive and childlike.

Taylor (103), Orne (66), and Crasilneck and Michael (10) conducted more extensive studies using normal subjects. Taylor's 12 subjects produced Goodenough drawings at the suggested ages of 6, 8, and 10 years, both under hypnosis and in the waking state. His attempt to distinguish clearly between "true" and "simulated" regression resulted in the following conclusions: (a) the total group showed "no significant difference between the hypnotic and waking drawings"; (b) the group that could be deeply hypnotized by the criteria of the Davis and Hus-

TABLE 5

GOODENOUGH IQs AT DIFFERENT REGRESSED AGES
After Taylor (104)

Part A	No. of cases	Regressed age					
		Under hypnosis			Waking simulation		
	12	6	8	10	6	8	10
		132	124	112	128	127	119

Part B	At age 6	At age 8	At age 10
Correlation of drawing MAs with Stanford-Binet MAs (39)	.832	.557	.849

band (11) scale "showed a greater average deviation of the three drawing IQs in the waking than in the hypnotic drawings"; (c) the lightly hypnotized group showed the reverse; and (d) deeply hypnotized subjects showed more primitive motor coordination and features in their drawings than did the lightly hypnotized subjects. Since primitive and mature features appeared concurrently, Taylor felt that he had no evidence for a simple return to prior memories. An examination of Taylor's data reveals that the IQ tended to fall with increasing suggested age as shown in Table 5A. For the hypnotically regressed condition the change was more marked than for the waking simulation. This could be due to the failure of the test with increasing age level, but as shown in Part B of the table, the correlations between the MAs of the drawing test and the MAs of the Stanford-Binet do not indicate this. It is more likely, therefore, that regression, hypnotic or waking simulation, failed to reach the suggested CA.

Orne (66) reached a similar conclusion in his study of 10 subjects regressed to age six. He found that drawings gave no evidence of true or complete regression and that the personality of the subject remained adult under suggestion.

A different type of drawing performance was used by Crasilneck and Michael (10). In the Bender Visual Motor Gestalt Test geometric designs consisting of simple figures are copied (2). This test was given to 10 student nurses 19 to 22 years of age. Four conditions were investigated: (a) awake, (b) awake with instructions to pretend to be four years old, (c) hypnotized with instructions to pretend to be four years old, and (d) hypnotized and regressed to four years. Three raters, unaware of the nature of the experiment, rated the maturity level of the Bender drawings in half-year steps from 3.5 years to 11.5 years. Rating reliability was high. The mean motivational age levels for the four conditions were: (a) 11.2 years (awake), (b) 9.9 years (awake-pretend), (c) 7.8 years (hypnotized-pretend), and (d) 7.3 years (hypnotized-regressed). These differences, except for the comparison of (c) and (d), were significantly different statistically, and all were significantly different from a hypothetical mean of age four. The raters also ranked each of the four productions of the 10 subjects. The mean ranks from (a) to (d) were 1.1, 2.0, 3.3, and 3.6. All differences were statistically significant except that between (c) and (d). The judges' clinical opinions are interesting. They

said that some subjects were adults, some were children, some were psychotic, and some were cases of organic damage. In any event, the performance under either hypnotic state was not typical of what is normally found in the 7.3-7.8 year age range for children. Orne also obtained an independent evaluation of his subjects' drawings from Dr. Karen Machover, whose opinion is worth quoting again. "The drawings definitely do not resemble those of six-year-olds. Mature and immature features are intermingled, and in certain respects the drawings in regression are more mature than those done in the waking state. The term 'sophisticated oversimplification' seems appropriate for these drawings" (66).

Handwriting. Like drawing, handwriting is a performance that develops progressively over the years. Krafft-Ebing (52) first noted the changes in handwriting in a woman at hypnotically regressed ages from five to 20 years. Gakkebush similarly reported that handwriting samples showed childish characteristics under age regression, and claimed that old specimens obtained from parents were "completely identical" with productions under hypnosis (24, 25). Dolin's paper of the same period also showed several samples of childish penmanship under regression, but Dolin felt that leakage from other memory systems contaminated the reproductions (14). Suslova (102) obtained specimens from three subjects and found that the early samples took longer to write than the later ones; also, the copy-book style of the schoolchild was seen to degenerate at the later regressed ages into the illegible hand of the adult. Orne (66), on the other hand, examined the handwriting of his subjects under hypnotic regression and was impressed by the adult characteristics present.

Other Psychological Studies. The best single source of miscellaneous studies on hypnotic age regression is LeCron (55). It is especially interesting because it contains a number of personal communications from other workers, several of which have been mentioned above, as well as LeCron's own observations that are not published elsewhere.

An experiment by True (105) is remarkable. First, for the large number of subjects—fifty. Second, for the simplicity of the measure employed, which was simply the request for the day of the week at Christmases and birthdays back through the years. Forty men and 10 women between the ages of 20 and 24 were regressed to 4, 7, and 10 years. The days of the week were easily checked with a 200-year calendar. A test in the waking state revealed that "an extremely small percentage gave correct answers to any of the questions, leading one to believe that when correct answers were given they were largely owing to chance." This percentage should have been about 14. The results under hypnotic regression are shown in Table 6. For all ages the average recall was 82.3 percent correct days. True's data were not corrected for chance success, but it is obvious from the table that doing so does not change the outcome of the experiment. There is progressively better recall with age, and young children appear to be more impressed with Christmas than with a birthday. The latter finding is in accord with the report of hypermnesia under hypnosis for emotionally charged material (83).

Reiff and Scheerer (82) wondered that four-year-olds should be enough aware of dates to record the memories elicited by True and Sutcliffe (102) in his review of age-regression suggested that adults could readily determine the correct day of any date by use of a formula. This seems unlikely.

TABLE 6

PERCENT CORRECT RECALL OF 100 MEMORABLE DAYS UNDER HYPNOTIC AGE REGRESSION

After True (105)

Regressed age	4		7		10	
Day recalled	Birthday	Christmas	Birthday	Christmas	Birthday	Christmas
Uncorrected for chance	62	76	84	86	92	94
Corrected for chance[1]	56	72	81	84	91	93

[1] $p = \frac{(p' - c)}{1 - c}$, where p' is the obtained percent and c is the probability. In this case c = 0.143.

Dittborn (13) applied True's test to only one subject, getting two correct responses out of six, and Best and Michaels (4) obtained largely negative results in a pilot experiment that had some points in common with True's method. They hypnotically regressed five subjects of 21 to 25 years to two past birthdays that fell on either Saturdays or Sundays, i.e., non-school-days. Four subjects incorrectly claimed to have attended school on both birthdays, and one subject said she went to school on one birthday but not the other. Out of 10 responses, nine were wrong. The subjects were also given a digit-memory-span test. The authors state, "At the regressed levels all the subjects responded appropriately on the digit-span test, failing levels associated with higher age groups." The digit test finding was given as evidence that the subjects were regressed to an extent, and it was concluded that "age regression is not true or complete. Subjects cannot return under hypnosis, at the will of the hypnotist, to *any specific* time or place in their personal history" (their italics).

When Reiff and Scheerer (82) asked their five hypnotic subjects for the day of the week of their seventh and tenth birthdays they got four correct answers out of ten. Only one of ten control subjects gave the correct answer as an admitted guess.

Gakkebush et al. (26) presented to their subject a word-association test at seven regressed ages between four and 12 years. Reaction times on responses to 10 words shortened with increasing age, and the responses became more appropriate to the stimulus words.

Maiorov and Suslova (59) analyzed the development of speech patterns under hypnotic age-regression. Five subjects of varying psychopathology between the ages of 21 and 45 were regressed to ages from one to 10 years. The subjects were tested by being required to pronounce words given by the experimenter. Childish speech patterns, shown by elision, sound substitution, and the like, were present from one to five years, and appeared to follow the normal development. From six onward the words were repeated in adult form.

In another study Maiorov and Suslova (60) regressed a 47-year-old woman with hysterical symptoms. At the suggested ages of one to two years some childish behavior was observed, mixed with more mature responses. Behavior was less ambiguous at the suggested ages of 5, 14, 16, and 35 years, where the most objective test applied was taking dictation. Since

the woman learned to write before the Russian orthography was modified in 1918, she used some of the old letters in her spelling when regressed to a time before this date. Age progression to 70 and 75 years yielded more convincing behavior than was found at the regressed ages of one to three.

Suslova (101) has reported success in distinguishing between mental performance in the waking state and the somnambulistic phase of hypnosis by measuring the speed and errors made in the simple addition of digits. As compared with the waking condition, the results on six subjects under hypnosis showed a decrement in the number of sums performed in 5 min. samples and an increase in the number of errors. This test, considered to be an index of the functional capacity of the cerebral cortex, was used by Suslova (101, II) in a second paper on age-regression. Ten subjects between the ages of 18 and 44 with mild psychopathology were tested in the waking condition, the hypnotic condition without age suggestions, and at a variety of regressed ages down to seven years. The development of the ability to add facilely was readily apparent in the results. At the early suggested ages the performance was lowest, later it approximated that of adulthood. The data under hypnotic regression were compared with performance under non-regressed hypnosis, since it had already been shown that fewer additions were made in hypnosis than when awake. At some ages a factor of five or more separated the regressed from the normal hypnotic performance. Some interesting sidelights were reported. One, reminiscent of Erickson's (18) case, was found by accident. The test performance of a 37-year-old woman fell at age 30 to below what it was at age 10. It was discovered that she was 30 years old when knocked unconscious during a bombing at a train station while at the front during World War II.

Finally, Reiff and Scheerer (82) report several experiments in which specific memories were sought by means of questionnaires given in the waking and age-regressed states. The questions included such items of childhood information as names of teachers, names of fellow students in classes, and names of textbooks. The results showed that "when the experimental subjects were regressed to ages ten and seven, there was a statistically significant greater percentage score of memories than in the normal waking state. In contrast, there was no statistically significant increase in the percentage score of memories for the control subjects when they simulated ages ten and seven." Some of the replies could be validated from school records. The result showed that the experimental subjects recalled more valid data under age-regression than in the waking state. The simulators, in general, recalled so little that no attempt was made to check the validity of their memories.

DISCUSSION AND CONCLUSIONS

While much attention has been given to the spatial storage properties of the brain, the temporal ordering of memory traces is just as much of a mystery (21, 33). In either case, memory phenomena in living organisms have long been thought to depend upon modifications that take place in neural tissues. Nevertheless, despite the abundance of theories, no physiological mechanism has yet been demonstrated that accounts for either short- or long-term memory. Galambos and Morgan (27) have recently reviewed neurophysiological theories such as have been suggested by nearly every worker who has experimented on the brain. Today, in particular, much attention is being paid to the findings reported by Penfield, who dis-

covered in 1931 an area on the temporal cortex of epileptic patients where an electrical stimulus may reproduce a memory sequence recorded at an earlier time (71-77, 79).

The views held by psychologists on learning and memory are also varied (1, 34, 82), and foremost among them are the interference theory of forgetting of the behaviorists, and the trace alteration theory of the gestalt school. Both have been considered in detail by Osgood (68) with the conclusion that neither theory accounts for all the facts of memory or has been adequately tested.

A fundamental problem about the neural changes that occur in learning concerns their stability and permanence. At one extreme is the suggestion that the deposits we call memory, once laid down in the nervous system, are retained indefinitely in unaltered form (73-77, 79). This regards memory as systems of changes that are filed sequentially in time and are generally inactive. These memory systems are interconnected, however, and may be energized if one has the proper key. The key discovered by Penfield is the electrical reactivation of the ganglionic record mediated by mechanisms presumably in the temporal cortex and the hippocampus (78). In the Bekhterev-Pavlov model the key is the conditioned response.

As against this view it may be contended that all memories are subject, in varying degrees, to progressive alteration, and the decay shown in the curves of forgetting. In this process some of the material is irretrievably lost. A common-sense position lies somewhere between. Certain experiences are evidently impressed with sufficient emphasis to be retained for life. Others, like those needed to repeat a sequence of digits in the memory-span test, may endure for only seconds. Psychologists often refer to motivation as the force that contributes greatly to the difference between remembered and forgotten events, but only recently have cerebral structures been proposed that may one day provide a neural basis for motivation as a mechanism in memory (85).

Normal Recall

It is a commonplace observation that some very remote events in the past can be recalled with seeming clarity. This is accomplished by a process during which one attempts to search old memory files using his own keys. We are never sure that such recollections are free from the taint of recent, perhaps even frequent, reenforcement, and seldom can they be objectively verified or dated in a manner to satisfy the skeptic.

Studies on the normal recovery of old memories are scarce. Ebbinghaus (16) reported relearning a large, but unspecified, number of stanzas from Byron's *Don Juan* after 22 years with a saving of seven percent. Titchener (104) after 46 years relearned the 27 eight-line stanzas of a poem by Milton in 14 readings. Unfortunately he had no recall of how long it took to learn the poem as a 10-year-old schoolboy, but it seems reasonable that it was greatly in excess of this. During the first reading he recognized clearly some parts of the poem. Bartlett (1) has clearly shown the alterations that occur in memory systems after the passage of from a few minutes to several years.

Burtt's (6, 7, 8) study on early memory stands alone as a controlled experiment. Nonsense material consisting of three 20-line selections of Greek iambic hexameters were read daily to a 15-month-old child. Three new selections of the same material were read during each three-month period until age 36 months, for a total of 21 selections. At age 8.5 years one third of the original selections, and three new selections for control, were learned.

Then, at ages 14 and 18 years the second and last thirds of the original material were learned with comparable control material. A comparison between the trials required for correct recitation of the original and control material showed 27 percent saving at age 8.5 years, eight percent saving at 14, and zero at age 18. In Burtt's words, "Apparently the last four years were sufficient to eradicate completely any trace of the original stimulation during infancy" (8).

Hypnotic Recall

Evidence for hypnotic recall, like that of normal recall, suffers from the lack of verification based on independent criteria. Rarely does one find in the literature unobjectionable proof of the age of a memory trace. The situation is better for the unconditioned responses, but the reappearance of these under hypnosis may well lie out of the bounds of "recall," as ordinarily understood. Nevertheless, since some of the best objective evidence for regression is the recovery of reflexes, and since this process is intimately related to memory, it is necessary to examine it.

Conclusions from the Physiological Work. It seems clear that the reappearance of involuntary neural mechanisms that existed at pre-adult age-levels must be taken as evidence of regression type II, in Weitzenhoffer's terminology. The best indication that any such mechanism exists is found in the alteration of the adult plantar reflex. Here we have three cases observed by Gidro-Frank, five by True and Stephenson, three by McCranie et al., one by Sarbin, and an unspecified number by LeCron. McCranie et al. make it clear that dorsiflexion of the great toe is not recoverable in all regressed subjects, and other considerations indicate that a positive Babinski will occasionally appear in other states, such as fatigue and sleep. The problem is further complicated by lack of complete agreement among investigators as to the identifying criteria for this response. Nevertheless, the apparent fact that dorsiflexion appears rather abruptly at about five to six months argues strongly for a sudden decoupling of this reflex from the control normally exerted via the mature pyramidal tracts. The one reported observation of the Moro reflex also falls in the category of type II.

The sucking response, on the other hand, could be type II, but is so likely to be contaminated with adult knowledge of infant behavior as to be suspect. It has been observed often enough to be admitted as good evidence, but rather of type I at worst, and type III at best.

Except for one case, there is no satisfactory evidence that the infant pulse rate, which averages about 120 beats per min., can be reconstituted. The results from the EEG, if one excepts the single inconstant case of Schwarz et al., are solidly negative.

It is perhaps not surprising that an infantile postural reflex like the Babinski can be recovered, whereas the infantile pattern of brain rhythm cannot. In the one case, the original pathways still exist, and their function has been altered only by the inhibitory influence of neural mechanisms that have matured at a later date. Where these influences are removed by disease, for example, the old pathways control behavior as before. In the case of the EEG, however, we must conclude that the immature pattern has been replaced, not modified, by the adult pattern. We may not recover, then, what no longer exists.

The amazing thing about the disinhibition of the postural reflex is the fact that it is accomplished via informational channels that depend on words: learned material that is new in comparison with the age of the infant reflex arc. The effective suggestion

that an individual is five months old and not six operates through the use of learned verbal material that did not, and so far as we know could not, exist at the ages of five or six months. How can a cut-off sharp enough to release dorsiflexion at one suggested age, and not another, be mediated by learning that took place five or more years later? Perplexity over this problem is not new. Both Gorton (31) and Zeckel (119) have considered it in passing without coming to any conclusion. Zeckel found scant solace in Platonow's reflexological explanation, "If a conditioned stimulus can call forth an elementary conditioned response, the same stimulus can bring to life a large complex of chain reflexes with which it is associated in some way. A special activity can in this way be ascribed to the word, which is a complex conditioned reflex stimulus calling forth multiple complex reactions. Thus the complex of the associated engrams, embedded mosaically in the brain cortex and inhibited by the superposing and newly acquired ones, may be reinstated and reproduced with all the accompanying physical and chemical changes" (81). This might apply to memory mechanisms dating back to the period when words became conditioned stimuli, and, to be sure, this is the general context in which Platonow's remarks were made. Our problem is: what words could possibly have been conditioned to the postural reflexes at the age of six months? The most detailed accounts of how the first and second signal systems of Pavlov (37, 70) relate to the weakened cortical states said to be at the basis of hypnotic regression are given in the papers of Gakkebush (25, 26) and Dolin (14). Yet, there is nothing here that solves this problem. Until we have solved it, we cannot date verbal learning by ascribing to it the same age of the reflex evoked by it.

Conclusions from the Psychological Work. The use of Weitzenhoffer's terms for regression types is extremely difficult when applied to the psychological studies. Indeed, if the absence of leakage from late memory systems into early ones must be proved before a result can be accepted as evidence of regression type II, then one can say at the outset that there is no experiment to date that qualifies. However, an attempt will be made in the following to be less unbending.

The work on the recovery of recent memory material is summarized in Table 7. It is clear that meaningful and emotionally stressed material is more readily available under hypnosis than in the waking state. Indifferent material is not. However, in either case recovery is not complete—there is always some loss, just as there is in normal recall processes. It may be objected that testing for memory under the conditions of these experiments is not true age-regression, in that precise suggestions that recall be made at an earlier age were not given. However, as was said earlier, regression may be implied, and there is no logical reason for holding that a form of regression, i.e., the reactivation of specific old responses, is not involved. If admitted as regression, it seems to be more akin to regression of type II than a matter of role-playing.

Table 8 brings together the results of the various psychological tests that have been used to recover remote memory. It summarizes a motley lot of conclusions from experiments that are mostly inadequate from the standpoint of number of subjects, controls, and the use of objective criteria. First should be considered the sizeable experiments of Kline, Sarbin, and Young using intelligence tests (40, 88, 118). Young and Sarbin both showed that role-playing can produce an acceptable approximation of an MA at the suggested CA. Kline did not show that

TABLE 7

SUMMARY OF THE RESULTS COMPARING HYPNOTIC AND WAKING RECALL OF MATERIAL RECENTLY LEARNED IN THE WAKING STATE

Investigator	Type of Material	No. of Subjects	Delay between Learning & Recall	% Gain due to Hypnosis
1. Young (114)	Nonsense syllables	19	0 (?)	approx. 0.0
2. Huse (35)	Nonsense syllables	8	24 hrs.	−3.0
3. White, et al. (112)	Nonsense syllables	8	24 hrs.	−15.7
4. Rosenthal (83)	Nonsense syllables	13	24 hrs.	−1.2
5. Mitchell (63)	Three place numbers	2	5-7 min.	−30.0 for savings in trials 6.0 for savings in errors
6. Stalnaker & Riddle (100)	Poetry	12	1 yr. or more	53.7
7. White et al. (112)	Poetry	8	24 hrs.	53.1
8. Rosenthal (83)	Poetry	13	24 hrs.	24.0
9. White et al. (112)	Motion picture scenes	8	Av. = 2.5 days	83.1
10. Young (114)	Adj.-noun assoc.	19	0 (?)	approx. 0.0
11. Young (114)	Logical sequences	14	0 (?)	approx. 0.0
12. Young (115)	Misc. objects	2	?	approx. 0.0
13. Sears (95)	Misc. objects	24	0-1 wk.	10.9 - 25.0
14. Rosenthal (83)	Words mixed with nonsense syls.	13	15 min.	−1.0
15. Rosenthal (83)	Words mixed with nonsense syls. plus tension	13	15 min.	39.0
16. Rosenthal (83)	Poetry plus tension	13	10 min.	−0.4
17. Rosenthal (83)	Emotional mixed with neutral words	13	24 hrs.	0.03
18. Rosenthal (83)	Verbal & motor tasks plus tension	13	0 (?)	17.9

his subjects could not have role-played adequately in his experiment. Nevertheless, Sarbin found that the depth of hypnosis correlated with the accuracy with which the appropriate MA was reached. The most reasonable conclusion is that the results from intelligence tests demonstrate regression type III.

Next should be considered the carefully controlled experiments of Reiff and Scheerer (82) using a battery of performance tests. The results, fortified also by their findings on the recall of validated items from a questionnaire, clearly show the superiority of hypnotic recall of old memories, and offer little support for the role-playing hypothesis. They do not demonstrate the absence of interference from later memory systems, to be sure, but Reiff

TABLE 8

RESULTS OF PSYCHOLOGICAL TESTS GIVEN DURING HYPNOTIC AGE REGRESSION

Investigator	No. of Subjects	Test	Results
1. Gakkebush, et al. (26)	1	Binet-Simon	MA appropriate to CA
2. Platonov & Prikhodivny (80); Platonow (81)	3	Binet-Simon	MA too high for CA
3. Young (117)	10	Stanford-Binet	MA too high for CA
4. Sarbin (88)	12	Stanford-Binet	MA too high for CA
5. Kline (40)	10	Otis	MA appropriate to CA
6. Keir (38)	1	Stanford-Binet	MA appropriate to CA
7. Leeds (56)	1	Stanford-Binet Vocab.	Definitions showed maturation of intellect
8. Kline (41)	1	Wechsler-Bellevue	MA slightly high for CA but within experimental error
9. Spiegel, et al. (99)	1	Stanford-Binet	MA sometimes too high or too low for CA
10. Gakkebush, et al. (26)	1	Rorschach	Personality development traced by test
11. Orne (66)	10	Rorschach	Incomplete regression, personality remained adult
12. Sarbin & Farberow (91)	2	Rorschach	Regressed role consistent with adult role
13. Bergman, et al. (3)	1	Rorschach	Test followed clinical data
14. Mercer & Gibson (62)	1	Rorschach	Normal personality for regressed ages
15. Norgarb (65)	1	Rorschach	Personality development traced by test
16. Keir (38)	1	Rorschach	Personality unchanged by regression
17. Kline & Haggerty (50)	1	TAT	"Neuropsychological validity" of regression upheld
18. Taylor (103)	12	Goodenough	Primitive and mature features present together
19. Orne (66)	10	Modified Goodenough (?)	Incomplete regression, personality remained adult
20. Bergman, et al. (3)	1	Goodenough	Rough indication of primitivity
21. Mercer & Gibson (62)	1	Goodenough	MA appropriate to CA
22. Crasilneck & Michael (10)	10	Bender	Regressed drawing atypical of children
23. Kline & Guze (49)	1	HTP	Regressed drawing more primitive than adult
24. Gakkebush (25)	1	Drawing & handwriting	Regressed performance identical to original childhood samples
25. Dolin (14)	2	Drawing & modeling	Regressed performance modified by later experiences
26. Suslova (101)	10	Speed of adding	Regressed adding rate typical of children
27. Reiff & Scheerer (82)	5	Eight Piaget-like tests	Regressed performance consistent with suggested age level

and Scheerer appear to be getting closer to type II than the mixed type III.

The results on the Rorschach from the two largest studies, two and ten subjects, are negative. Both Orne (66) and Sarbin and Farberow (91) found that adult personality patterns permeated the regressed productions. The other Rorschach studies are interpreted by their authors as verifying age-regression in varying measure. Sarbin's position, however, is that a form of role-taking is at the very basis of Rorschach responses made under instructions of hypnotic regression. This argues against accepting even the last part of LeCron's statement, when, in the introduction to Norgarb's study he says, "Rorschach tests offer an excellent means of judging the actuality of hypnotic age regression, for reaction to such a test is difficult if not impossible to simulate" (55). The Rorschach, like intelligence tests, doubtless taps old memories, but, as mentioned earlier, the Rorschach is not well suited for accurately dating their ages. The conclusion again is that performance on the Rorschach is a case of type III regression.

The drawing test of Goodenough has been standardized to the extent that MAs from drawing scores correlated fairly well with those of intelligence tests (30). When given by Taylor (103) to the largest sample tested with drawings the results were negative. The next largest sample was tested by Orne (66), using drawings that contained elements of the Goodenough. The results were negative. The results of the Bender by Crasilneck and Mitchell (10) on 10 subjects were also negative. In the few positive cases, all on one or two subjects, it is likely that most adults would know enough about the simplicity of children's drawings to make this kind of test result suspect. In any event, drawing must be put down as type I or III regression.

Suslova's (101) adding test was not scaled to age and therefore does not date the ages of the memories on which the performance was based. Further, there is no reason to believe that type II regression was demonstrated. The slower adding rates of early childhood could have been simulated.

In the physiological studies the plantar reflex provided a response difficult to explain away. With the psychological studies it is True's (105) experiment on the recall of dates that appears to present remarkable evidence for the reactivation of old neural connections. If confirmed, this work would seem to qualify as regression type II. Unfortunately, it has not been confirmed, and the contradictory results of Best and Michaels (4) and Reiff and Scheerer (82) leave the matter unresolved.

Validity of Hypnotic Age Regression

The genuineness of hypnotically induced age regression is a much debated theoretical matter. From the viewpoint of the hypnotherapist, the questions of "exact chronological duplications" (93), and the precision attained in memory recovery (97) may be quite academic. He has too often seen that hypnotically aided recall has been useful, especially when the patient has been allowed freedom in regression. Nevertheless, the question of the validity of age-regression deserves some kind of answer. Gill (29) points out that it depends on what one means by genuine. Speaking of a clinical case of spontaneous regression he says, "If by genuine regression one means a state such as was described by revivification,[7] we feel there is no such thing; but if by genuine regression one means a significantly altered ego state, oriented to and integrated in a previous time in the patient's life history, but

[7] I take this to mean regression type II.

with the retention of a relationship, conscious or unconscious, to current reality, this case shows that such a state can develop spontaneously on the induction of hypnosis. This spontaneous regression showed many of the features described in accounts of induced hypnotic regression. It is true that some instances of induced regression are more 'complete' than the one here described and that in them inconsistencies as glaring as answering questions about matters subsequent in time to the regression do not occur. The reasons probably lie in the careful instructions with which such states are induced." However, others have shown that even in carefully controlled regressions, leakage from later memory systems occurs (10, 66, 88, 103). There can be no doubt that some old memories are revealed by hypnotic age regression just as some are recovered during well-motivated efforts to recall in the waking state. The fundamental question is: Which state is superior? Nearly twenty years ago White (111) wrote that a theory of hypnosis must explain, among other things, the fact "that the hypnotized person can transcend the normal limits of volitional control . . . . " Offered in partial support of this was evidence for the improvement of recent memory in the trance state. This evidence is still in good standing, although it has been shown that it is largely meaningful and emotionally stressed material that is retained. Reiff and Scheerer (82) also present evidence for transcendence in their investigations of more remote memory systems. Orne (67) has recently found reason to be skeptical of the claim that normal physiological capacities are transcended in the hypnotic state, and in general the present review affords little evidence for type II regression. On the physiological side, there is indeed the remarkable and as yet unsettled case of the unconditioned plantar reflex, but the psychological evidence for transcendence in the exact reinstatement of old neural deposits due to learning is still hardly convincing. While Kline (42, 46, 50) believes that the "neuropsychological" validity of age-regression has occasionally been demonstrated, there is nothing in the data to uphold the view that specific memory patterns may be unfolded with precision by hypnotic means. This is to say that there is nothing to show that memory systems are filed temporally in layers, impervious to the ravages of time. This is precisely the conclusion Young (118) reached two decades ago, and recently affirmed by Sutcliffe (102).

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WHAT IS HYPNOSIS?

by William T. Heron[1]

The title of this paper suggests a rather ambitious project. We commonly say that hypnosis under different names has been known for centuries. In making this statement we are perhaps inadvertently neglecting the fact that the phenomena indicated by the various names previous to the term hypnosis, and even the phenomena indicated by the name hypnosis, have not through these many centuries been identical. Presumably, however, there must be some common element running throughout the ages in all these situations, no matter what the name, which permits us to say that hypnosis has been known from the earliest times.

Is there something which is intrinsically characteristic of the hypnotic state? Are there some phenomena or a constellation of phenomena by means of which we can identify hypnosis even though we do not know how the individual arrived at his present state? Are there *unequivocal* diagnostic signs of hypnosis? Is there an "essence" of hypnosis?

There are those who have answered this inquiry in the affirmative. Braid, for example, describes the following syndrome as listed by Weitzenhoffer, Gough, and Landes (1) in a recent paper, which they name the "Braid effect":

"1. Pupil size: First a constriction (due to consensual reflex), then a dilation.

"2. Pupil motion: Braid speaks of a 'wavy' motion of the pupil without further details.

"3. Eyelid closure: May occur spontaneously. If not, the bringing forward and downward by the experimenter of the extended and slightly separated fore- and index finger from the fixated object to the subject's eyes will usually cause eye-closure. Eyelid closure tends to be spasmodic, and the eyelids show a vibratory motion. In case this last procedure fails to cause the eyes to close, one then tells the subject to allow his eyes to close the next time this is done, but to maintain his eyeballs in the same position and his mind riveted on the thought of the object.

"4. General tonic rigidity: Body members resist passive movement.

"5. Specific catalepsies: Primarily in the form of waxy flexibility, observed when manipulating a member, with rigidity setting in. The latter may spread.

"6. The pulse rate rises markedly following the production of a specific catalepsy.

"7. Hypersuggestibility: Can be demonstrated by describing to some other person fictitious effects which are to be elicited in the subject. Thus Braid demonstrated the various presumed (by the mesmerists) effects of magnets with a piece of wood by describing these effects to an observer near the hypnotized subject.

"8. Stupor: If the Braid effect is allowed to persist without further stimulation it becomes transformed into a stuporous state in which the subject becomes unresponsive."

Others have said that there is nothing specifically characteristic of hypnosis. Bernheim (2), for example, implied this when he said: "There is no hypnosis but only suggestion."

Many of us have written articles and books on hypnosis without coming to grips with the question of precisely what it is that we are writing about. Perhaps this is just as well, because if we tried to give a concise, abstract, and meaningful definition of hypnosis we would likely get so discouraged that our paper or book would never be written.

In one of the most recent books, Marcuse's "Hypnosis — Fact and Fiction," the author makes a half-hearted attempt to define the term hypnosis (3). He says: "All things considered, it may well be better to define 'hyp-

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nosis' by what it does rather than by what it is. For the present, we may tentatively define hypnosis as an altered state of the organism originally and usually produced by a repetition of stimuli in which suggestion (no matter how defined) is more effective than usual."

This definition is obviously ambiguous. Does the author mean that the condition of an individual in which "suggestion is more effective than usual" is always to be called hypnosis, regardless of its historical antecedents? If so, then individuals who become more than usually suggestible after a drink or two of an alcoholic beverage would be in the hypnotic condition. Or does he mean that we must always take into consideration the causal antecedents of the situation? In the latter case we could not know whether the individual is in the hypnotic state until we had investigated the history of his present condition. The latter procedure is not unusual. There are a number of diseases which are difficult to diagnose in the absence of a case history. On the other hand, some diagnoses can be made completely independently of historical evidence; e.g. smallpox, typhoid fever, and many others. I rather think that all professions dealing with human diseases and difficulties would prefer to be able to make a diagnosis without recourse to a case history. And I think that we would also prefer to be able to identify hypnosis without having to know how the individual arrived at his present state.

On the other hand, if there is nothing peculiarly characteristic of what we name hypnosis and the whole matter revolves around suggestion as indicated in Bernheim's statement, then perhaps we would be better off to discard the term hypnosis altogether and concentrate our efforts on investigations of the factors which determine the degree of acceptance of a suggestion. Otherwise we may be trying to investigate and study something which does not exist.

Bernheim is not alone in stressing hypersuggestibility as the essential characteristic of hypnosis. Hull (4), Marcuse (3), and many others do the same. Warren (5), for example, gives this definition: "an artificially induced sleep, but physiologically distinct from it, which is characterized by heightened suggestibility, as a result of which certain sensory, motor, and memory abnormalities may be induced more readily than in the normal state."

Let us examine these definitions which involve suggestion and suggestibility as the determining factors in hypnosis. First, we again have a problem of definition. There is, however, much more general agreement on the definition of these terms than there is in respect to the term hypnosis. I like Warren's (5) definition of suggestion as "a stimulus, usually verbal in nature, by which one individual seeks to arouse action in another by circumventing the critical, integrative functions." Suggestibility he defines as "a more or less permanent susceptibility to suggestion." McDougall (6) gives a somewhat similar definition when he says: "Suggestion is a process of communication resulting in the acceptance with conviction of the communicated proposition in the absence of logically adequate grounds for its acceptance."

If hypnosis is simply an increased susceptibility to suggestions, it follows that all the preliminary conversation with the patient and the induction methods themselves are devices aimed at reducing or circumventing the "critical and integrative functions" of the patient. Since this circumvention is easier to accomplish in some patients than in others, we say that some are more suggestible than others, at least at the moment when the practitioner is working with the patient.

If hypnosis is simply the uncritical acceptance of an idea, wish, desire, or motive, then our study of hypnosis should include not only methods of reducing the criticalness of the subject, but also we should study the extent to which behavior and physiology can be influenced by the convictions of the patient. One sometimes hears a physician say that he fears for a patient because, as the physician puts it, the patient has lost "the will to live." Can the conviction that death is near or that death is desirable actually produce death, even in the absence of disease? If the answer is "yes," as many authorities believe, then we perhaps have the ultimate example of the power of firmly accepted beliefs and ideas.

If we look upon hypnotizing as a procedure for the circumvention of criticalness and also understand the tremendous influence upon a patient's behavior of the ideas and belief which he already has when he comes into our office, then we can understand why it is that we have so much trouble helping some patients to go into the hypnotic state. Some writers have used the phrase "determining tendencies" to indicate the ideas, wishes, desires, notions, and motives which affect a person's behavior. The determining tendencies which are more numerous and firmly held will obviously be harder to circumvent, even though the circumvention is for the welfare of the patient.

Some psychologists and sociologists have become involved in what they call "role theory". They have worked hypnosis into this general theory, and Sarbin (7) puts it at step IV of a seven-step scale of roles. The one end of the scale is defined as:

Role and self well differentiated
Minimal involvement
Few organic systems
Little effort

The other end is defined as:

Role and self undifferentiated
Maximal involvement
Entire organism
Much effort

According to this theory, hypnosis involves the subject taking the role of a hypnotized person. Or, in other words, the subject is acting as he conceives a hypnotized person acts as guided by his own preconceptions and by the directions of the hypnotist. The subject "behaves *as if* he is blind, or deaf, or fearful, or analgesic, or whatever the specific instructions call for."

From this point of view presumably the differences in hypnotizability lie in either inadequate perceptions or deficient role-taking ability or both of these. In regard to the latter, Sarbin indicates that some people can act "*as if*" better than others.

In a recent article by Orne (8) it would appear that, so far as observed behavior is concerned, there is little if any difference whether one considers hypnosis as a matter of suggestibility or role-taking. He found that subjects who were instructed to act "as if" behaved in essentially the same way as subjects who were responding to suggestions.

In the light of this evidence, I would prefer to look on hypnosis as heightened suggestibility rather than role-taking. As of this moment I cannot see that the notion of role-taking or "as if" behavior adds at all to our understanding of hypnosis, and to say the least the terms suggestion and suggestibility are older and better defined.

We come now to a third concept or definition of hypnosis. It is the notion that there is nothing in the observable behavior of the subject or in the procedures called hypnotic induction which distinguish the hypnotic state from other states which a person may assume and that the only way in which hypnosis can be differentiated is in

terms of the subjective experience of the subject.

This is the essential position which is taken by Orne (8) on the basis of results from his experimental work. It is also consistent with the definition of hypnosis by White (9) as an "altered state of consciousness," and likewise with some of the concepts expounded by Pavlov (10), Weitzenhoffer (11), and others.

Orne found that there was no observable difference in the behavior of subjects who had been subjected to hypnotic induction procedures and those who were told to simulate hypnosis, or those who were simply asked to do certain things. As a matter of fact, he found that motivated unhypnotized subjects held a weight at arm's length longer than they did when they were hypnotized. Though he could find no observable behavioral differences characteristic of hypnosis, he did find that subjects in deep hypnosis gave verbal reports indicating a difference in subjective experience. These "changes in the subjective experience are characterized by (a) discontinuity from normal waking experience, (b) a compulsion to follow the cues given by the hypnotist, (c) a potentiality for experiencing, as subjectively real, distortions of perception, memory, or feeling based on 'suggestions' by the hypnotist rather than on objective reality, (d) the ability to tolerate logical inconsistencies that would be disturbing to the individual in the wake state."

If a change in subjective experience is the "essence" of hypnosis and if there is nothing peculiarly characteristic in the behavior of the hypnotic subject, then those patients who say to us "Nothing happened to me; I wasn't hypnotized" are correct, since they obviously felt no change in their subjective experience.

If this point of view should be substantiated by subsequent experimentation, then we could properly use the term hypnosis only in regard to the 20 per cent or less of patients who go into what we now call the somnambulistic state. For the other patients we would need a new term or none at all.

Of course there is at least one other way of answering the question: What is hypnosis? It is the method which most of us use, namely, to use a so-called scale of hypnosis and a method of hypnotic induction. In this case hypnosis is defined in terms of not only what the subject does but also what is done by the practitioner. In other words, if the practitioner uses a technique of hypnotic induction and the patient responds with behavior as indicated by the scale of hypnosis, we say that the patient is hypnotized to a certain level, depending upon where we are in the scale. This is an operational definition of hypnosis, but of a rather peculiar kind, since it involves the behavior of both the hypnotist and the behavior of the hypnotized. No doubt there will be changes in our scales and some changes in methods of induction with the passage of time. Such changes have no doubt already occurred in the history of the world. The behavior of Mesmer's patients was as a rule quite different from the behavior of ours. Therefore the content of our operational definition of hypnosis will change and has changed.

From the operational point of view we do not care whether the patient is simulating or faking, so long as we get the results which we seek for the benefit of the patient.

In practical work this operational definition of hypnosis is perhaps the one which we will continue to follow, at least until a great deal more research is done.

Nevertheless, there remains the fascinating question: "Is there an essence of hypnosis?"

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HYPNOTHERAPY OF MIGRAINE AND OTHER TYPES OF CHRONIC HEADACHE[1]

by Lester S. Blumenthal, M.D., F.A.C.P.[2]

Hypnosis will never rid a patient of his chronic, recurrent headache attacks. Neither will aspirin, codeine, morphine, ergotamine or any other single therapy or medication.

The treatment of any patient is never simple. There are so many factors involved in the mechanism of production of headache and in the life situation of the patient leading to the physiologic changes accompanying the headache that a thorough understanding of all of these factors is mandatory. Each patient must be evaluated as completely as possible. A thorough history, physical, neurologic, and laboratory examination must be carried out to convince not only the physician but also the patient that the cause of his trouble has actually been determined. Every fear of brain tumor, mental disease, or stroke that these patients harbor must be allayed. Once this has been done, and the patient understands what is going on in his head during a headache attack, and what has happened to him or in his environment to bring on the attack, he is in a better position intelligently to follow through with therapy. He then realizes that the physician has a sincere interest in him and his symptoms and desires to help him and that he will not be dismissed with the usual remark that "everyone has headaches and no one dies from them. Go home and take an aspirin." Therefore, the well-prepared patient is then ready, eager, willing, and able to listen to what we have to say and to follow directions or suggestions.

The vast preponderance of chronic, recurrent headache attacks are of two kinds: (1) migraine and related types of vasodilating headache; (2) tension headache due to prolonged anxiety, emotional upset, or other tensions leading to chronic spasm of the musculature about the head and neck. Hypnosis can be applied to both in several different ways and on various levels.

When a patient has had the mechanism of production of headache explained to him, he usually is able to detect well in advance when an attack will begin. At this stage he is able to recognize that the attack is imminent and does not have to await the development of severe pain or nausea to know he is headed for trouble. The various factors leading to the headache attack and the role of mounting tension preceding the attack in these patients are described in detail in one of our publications (1). We inform our patients what their limits are and emphasize that they must learn to recognize when they are exceeding these boundaries. With this information and the experience they have gained they are now able to anticipate the acute attack and institute therapy. Each patient is provided with various drugs and drug combinations and knows when and how to administer these. These often include sedatives, analgesics, or anesthetics. It is here that hypnotherapy can be applied. We teach our patients simple techniques of relaxation by means of self-hypnosis. He is told that when he is under pressure in any way, he should take these relaxing exercises at regular intervals throughout the day, in an effort to prevent mounting fatigue and tension. In this way prophylaxis of headache can be practiced. He is also taught that, in

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addition to whatever medication he might take, he also should sit or recline in a dark, quiet place and use relaxation techniques at the onset of any sign of headache. Most patients are able to do this quite well, once this has been demonstrated to them several times in the office. In addition to simple relaxation techniques, we also teach fractionation, in which they learn how to think of the tensions about their head relaxing and of the tight, painful muscles or the overstretched blood vessels being detached from them and left on a shelf across the room.

Any technique in which the patient will have to report to our office or call us on the telephone for therapy for each attack would be practically impossible in our practice, as we would never have time to handle them on an individual basis. Nevertheless, an occasional patient does get into severe difficulty, and hypnotherapy must be applied for an individual attack. This again can be done in the office or by telephone, as illustrated by the following case illustration.

A 39-year-old white housewife, who before her recent illness held a very responsible job in the government. She has been under my professional care since 1946 for severe premenstrual tension, dysmenorrhea, and migraine headaches, which had occurred every month for as long as she could remember. This was handled on a strictly medical level with sedation and analgesia, which kept her quite comfortable. She would lose one or two days' work each month and have no more difficulty until the next month. Early in 1957 she had much reconstructive dental work performed, could not eat, and lost a considerable amount of weight, which she has never regained. She came to see me primarily because of this loss of weight, but then told me that she had become increasingly nervous, apprehensive, and extremely fatigued, and she could not sleep. It was obvious that she was going through a mental depression. Since that time I have taken care of her medical needs while she has been under the care of an excellent psychiatrist and undergoing psychoanalysis, shock therapy, and the use of every new tranquilizer and psychic energizer. In spite of this, her headaches progressively increased so that they were becoming quite frequent and very severe. In addition she has had intermittent abdominal attacks, which to me are abdominal migraine, sometimes occurring with the headache, sometimes without. During one such attack she was hospitalized and had a thorough investigation for an acute abdomen. This was done because every sign pointed to a rigid abdomen with a possible ruptured viscus. During all this time, it became more evident that her problems were due to the fact that she had never been pregnant, was approaching the age of menopause, and lived with the fear that she would never conceive and be a mother and therefore had failed her mission in life. Coupled with this was an actual fear of pregnancy instilled in her as a young girl by well-meaning parents and relatives, who described some terrible things that had happened during pregnancy in other members of the family. It was this conflict that led to migraine in all of its manifestations. To compound this even more, her husband is an immature individual, who could never stand to face the responsibility of parenthood. While discussing possibly adopting a baby and then being faced with the realization that she never could do this because of her husband's immaturity, her symptoms became worse and have been severe ever since.

Over the years it became obvious that it made very little difference what was done to or for her during an attack of headache or abdominal distress. The fact that she could lean on us and knew that we could help her always brought her out of it. At first we used to administer strong sedatives, narcotics, or ergot preparations for her attacks with success. But the realization that she needed reassurance, support, and comfort more than the medicine made us first use hypnotherapy for these attacks. At times I use the persuasive, supporting, soft, reassuring technique, talking to her, soothing her, calming her down, and then have her close her eyes and relax her tensions with the suggestion that her head or stomach, whichever the case may be, will relax and feel much better as she relaxes. This works just as well whether she comes to the office, calls me on the telephone, or whether I am sitting by her bedside at home or in the hospital. On the other hand, there are times when she gets so anxious and agitated that she is practically hysterical and sobbing in the middle of a spell before we can get together. At such

times I have found the authoritative, domineering technique, during which I talk to her in a loud, strong, firm manner, is the only thing that will stop her hysterical sobbing and calm her down so that I can actually get to her. Since application of these techniques she has not required the excessive doses of sedatives and narcotics that were formerly used. Once her attack is settled, she relaxes, sleeps for several hours, and awakens refreshed, rather than being dizzy, groggy, and doped up for several days after the use of strong medication.

During the course of our examinations and discussions with the patient much information is disclosed. As described in the opening paragraph, the thorough examination and sincere interest acts to remove restrictions or inhibitions from the patient, and most of them then open up and pour out their problems to us. This is true in the waking state, and it can be accomplished in the hypnotic state. The patient will usually volunteer all the important information necessary for him and the physician to get a thorough insight into his background and the conflicts that have caused his anxiety, tension, or other emotional discord leading to production of headache attacks.

At times hypnoanalysis can be used to extract deep-seated information or to produce an abreaction. However, in my experience this is rarely necessary and rarely, if ever, acts as a cure. If one is tempted to probe deeply for the cause of emotional conflicts, he should indeed look long and hard at the problem before doing so. It is just this type of patient who usually is on the verge of a psychotic break and in whom the anxiety, tension and, yes, the painful headache itself often are factors in keeping him stabilized.

It is for this very reason that hypnotherapy is never used for simple symptom removal. We never tell a patient that he will relax and wake up without a headache. In explaining the physiology involved in his headache, we describe the relaxation of the muscle and the decongestion of the overstretched blood vessels as he relaxes, and we *infer* that he can actually feel the headache receding, as the physical causes of them are removed. In this way, if he wants to follow our line of reasoning and does so, then actually the pathophysiology will be reversed, and his headache will go away.

While most experienced practitioners are able to recognize the schizophrenic, paranoid, depressed or other psychiatrically abnormal individual, there are yet many subclinical states of these that will go unrecognized if we do not look out for them. At the present time, I am in the process of preparing for publication a series of case reports in which patients came to me with severe headache attacks, which turned out to be symbolic expressions of masked depressions, several having had actual suicide attempts. Hypnotherapy, during which their headache symptoms are removed without control of all other factors, is certainly contraindicated in such an instance. Two such patients were recently seen on one afternoon. One was a 39-year-old man, whose headache was an expression of his frustration, guilt, and anxiety over an intense homosexual affair. The other was a 25-year-old boy, whose headache had been constant and severe ever since he first became impotent at the age of 19. The description of this headache was so vivid that it actually was a perfect description of an orgasm. He described his head swelling progressively until it got so large, tight, full and congested that it felt it would burst at any moment. The only relief he obtained was when he actually had a feeling that it had burst, as with an orgasm. During his first visit to my office, using just interest and sincerity, he poured the whole story out to me, and also volunteered that whenever his head felt this way his penis felt as if it were shriveling up smaller and smaller into his body. Hypnotherapy was not attempt-

ed in this case. He was referred for intensive psychotherapy, which he is now undergoing.

In order to make sure that the patients understand, retain, and continuously put into practice the results of our discussions with them, I ask them to write down in their own words their interpretations of our findings and directions and also to record how they have put these into practice. The following letter was just received from a 42-year-old white housewife, who has done just that:

I have had headaches since high school. Last year it got to the point where I was sure I was losing my mind. The headaches were an almost constant thing, were of longer duration, and the pressure never stopped even when the pain did. My doctor said he could not cure them so sent me to the headache clinic.

Perhaps the most reassuring thing to know, after a complete physical, was that there was a clean bill of health and that, if I would, I could learn to control the headaches myself. I was assured that there are only three things in the head that can hurt, muscles, blood vessels, and nerves. Mine was the vessel. It (the vessel) was trained, over a long period, to react to a headache.

Tension, anxiety, and frustration cause them, not just worry.

My body has just so much energy to be used and when "things" use it over and above that amount, then comes the headache as a warning. I must learn to control my own life, to know what I can do comfortably and stop there.

To plan the days so there is time to spare for the unexpected things that come up and for things I enjoy doing.

Learn to say "no" occasionally.

Make a decision, and then let it go. If it is a wrong one . . . well, we all make mistakes. Don't dwell on it.

Put the day's routine on paper and cut it in half and then, if possible, that half in half.

In a year's time more will be accomplished than when every day was crammed full and then time taken off for a headache.

Don't feel I have to please everyone on the face of the earth.

It's hard to change a pattern of life after so many years.

I'm learning to say "no" and though it's still hard to do, gradually I'm able to make a decision and forget about it.

I do not write down every day's routine, but if it seems too much has to be done, I find if I write it down and cross off one thing at a time and forget the rest, the day does not get out of hand. Let go what doesn't get done.

The housework doesn't bother the way it used to.

I can even play golf with my husband without being out with a headache by the time we're finished nine holes. It is wonderful!

SUMMARY AND CONCLUSIONS

Hypnotherapy can be used in different ways in the treatment of headache. Simple relaxation techniques to prevent mounting anxiety and tension leading to headaches are most beneficial. Relaxation coupled with sedation, anesthesia or analgesia can be used either in the office or on the telephone by the physician during the patient's acute headache attack. The same techniques can be taught so that the patient, by himself or with the help of a relative or spouse, can apply these whenever necessary for recurrent attacks. Fractionation techniques, in which the patient concentrates on dissociating himself from his painful, spastic, throbbing head can be used in the more intelligent person. Hypnoanalysis, with production of abreactions, is rarely necessary, can often be harmful, and should never be used by the general physician without special psychiatric background, who might be unable to handle the consequences of an abreaction. Thorough history, physical examination, neurologic and laboratory studies, followed by diagnostic and therapeutic trials with various drugs to produce and to take away headaches are a prerequisite for a complete evaluation and understanding of each chronic, recurrent problem. Once this has been accomplished, the sincere interest and thoroughness shown by the physician is a most important tool in preparing the patient to understand and accept the explana-

tion of the diagnosis given to him as to the cause of his headache. He is then well primed, ready, willing and able to listen to and follow every direction and prescription that the physician has to offer. This naturally includes hypnotherapy, for which he is now fully prepared and actually *primed* by the above-described total approach.

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T.A.T. CARD 12M AS A PREDICTOR OF HYPNOTIZABILITY[1]

by Irving I. Secter, D.D.S., M.A.[2]

In a study by Ventur, Kransdorff, and Kline (2) the following interpretation was offered:

> We believe that the ability to see hypnosis in Card 12M [of the Thematic Apperception Test] may be related to a willingness to regard hypnosis as a useful therapeutic technique. It might also be urged that the reluctance or inability to recognize the two figures as parties to an hypnotic relationship implies an unwillingness to accept hypnosis in this sense.

This conclusion was based on an experiment in which the TAT protocols of 25 college students were compared with the protocols of 25 amputee patients in a hospital, all of whom were World War II veterans.

R. W. White (3) used the TAT Card 12M ("hypnosis card") in another manner. Seven judges predicted the rank of hypnotizability for 15 subjects from the protocols of the hypnosis card. A correlation of .34 ± .16 was found between the rank of hypnotizability and prediction of rank.

In connection with an investigation reported elsewhere (1) the author had an opportunity to compare the data from his experiments with the above. The subjects in Experiment I were 48 males, ranging in age from 24 to 70 years, from three occupational classes, dentists, physicians, and psychologists. All had previously failed to enter hypnosis while working with an experienced hypnotist. Six subjects were selected at random from volunteers at each of eight seminars on hypnosis held in different cities in the United States and Canada. Prior to a group induction procedure, all subjects were presented with TAT Card 12M and were requested to write a story about what they saw in the card.

The subjects in Experiment II were members of three elementary psychology classes. The classes contained 95 students (58 male and 37 female). The students were asked to look at TAT Card 12M and to write a story about it. After this, the students listened to a recorded hypnotic induction verbalization. The subjects were then tested for incidence of hypnosis and the depth of the trance state.

Analysis of Data from Experiment I

Were there differences in the incidence and depth of hypnosis between subjects who mentioned hypnosis in the TAT protocols and between subjects who did not mention hypnosis in the protocols? This was tested by chi-square.[3] The results are shown in Table 1. There is no significant difference between groups.

Differences in depth of hypnotic levels between those whose attitude to hypnosis was judged favorable by judges examining the subjects' TAT protocols and those whose attitude to hypnosis was judged unfavorable were analyzed by a chi-square test. The results are shown in Table 2. Majority opinions were used as a measurement of attitude. The neutral category was omitted, and depth levels of hypnosis were combined in order to yield a valid chi-square test, i.e., to insure a sufficiently large expected value for each

[1] This paper is part of a research thesis submitted to the faculty of the College of Arts and Sciences at Roosevelt University in candidacy for the degree of Master of Arts in the Department of Psychology. Donahue L. Tremaine, Ph.D., was chairman of the committee which read the thesis and was adviser for the project.

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[3] The chi-square used in analyzing these results is the chi-square test of independence. See Sidney Siegel, *Non-Parametric Statistics for the Behavioral Sciences,* New York, McGraw-Hill, pp. 104-107.

TABLE 1

DEPTH OF HYPNOSIS AS RELATED TO MENTIONING HYPNOSIS

Hypnosis in Protocols	Depth of Hypnosis				
	Refractory	Light	Medium	Deep	Total
Yes	5	5	16	4	30
No	1	6	7	4	18
Total. . .	6	11	23	8	48

chi-square = 3.50 df = 3 p > .05

cell. The results indicate no significant differences.

TABLE 2

COMPARISON OF HYPNOTIC LEVELS WITH JUDGMENTS OF ATTITUDES

Attitude	Depth of Hypnosis		
	Refractory and Light	Medium and Deep	Total
Favorable . . .	9	20	29
Unfavorable .	7	10	17
Total.	16	30	46

chi-square = .486 df = 1 p > .05

Analysis of Data from Experiment II

The difference in hypnotic susceptibility between groups who mentioned hypnosis in protocols of TAT Card 12M and between groups who did not mention hypnosis was analyzed by a chi-square test. The results shown in Table 3 indicate no significant difference.

Differences in hypnotizability between groups whose attitude to hypnosis was described as favorable by judges making judgments from TAT protocols and those whose attitude was described as unfavorable by the judges were analyzed by a chi-square test. As in Table 2, the neutral category was omitted and depth levels combined in order to insure a sufficiently large expected value for each cell. The results as shown in Table 4 indicate no significant difference.

The percentages of agreement between the judges, who judged the attitudes of S's toward hypnosis from protocols of TAT Card 12M were computed. The results were: between judges 1 and 2, 79%; between judges 1 and 3, 60%; between judges 2 and 3,

TABLE 3

DEPTH OF HYPNOSIS AS RELATED TO MENTIONING HYPNOSIS

Hypnosis in Protocols	Depth of Hypnosis				
	Refractory	Light	Medium	Deep	Total
Yes . . .	14	7	14	12	47
No	13	7	17	11	48
Total. . .	27	14	31	23	95

chi-square = .36024 df = 3 P > .05

TABLE 4

COMPARISON OF HYPNOTIC LEVELS WITH JUDGMENTS OF ATTITUDES

Attitude	Depth of hypnosis		
	Refractory and Light	Medium and Deep	Total
Favorable . . .	11	18	29
Unfavorable .	7	11	18
Total.	18	29	47

chi-square = .004277 df = 1 P > .05

58%. The variability in judgment probably contributed to the finding of no difference as shown in Table 4.

Discussion

From Table 1 it may be seen that five of the six refractory subjects in Experiment I reported hypnosis in the TAT protocols. Of the 30 who reported hypnosis, 25 were hypnotizable. Of the 18 who did not see hypnosis, only one was refractory. A chi-square test indicates a probability greater than .05 that no difference in hypnotizability exists between the group which reported hypnosis in the protocols and the group which did not report hypnosis.

The results shown in Table 3 are even more conclusive. Of the 47 who saw hypnosis in the protocols, 14 were refractory. Of the 48 who did not see hypnosis, 13 were refractory. These two groupings are almost identical in the distribution of hypnotic depth levels.

The above findings are in direct contradiction to a hypothesis that valid predictions of hypnotizability may be made on the basis of whether or not hypnosis is reported in the protocols of TAT Card 12M.

In the study by Ventur and others the hospital patients (experimental group) were found to be more resistant to hypnosis than the college students (control group). In the control group 64% recognized Card 12M as involving a hypnotic relationship between the central figures, whereas only 36% of the amputee sample did so. A "t" test showed this difference to be statistically significant at between the .02 and .01 levels of confidence. Such a result can be obtained by comparing non-comparable groups. It seems obvious that such was the case in the study by Ventur *et al.* It might have been more useful to compare for significance the differences within groups than those between groups. There is a strong possibility that such a comparison might have led to a different set of conclusions. The authors did not indicate whether an attempt was made to standardize the induction procedure for all subjects. The use of different procedures by different operators on different subjects in the same experiment introduces variables which can confound the results. It would seem to this writer that the use of TAT Card 12M as a general predictor of hypnotizability does not seem indicated on the basis of the data reported either by Ventur *et al.* or by the author. White (3) summarizes his study as follows:

> Seven judges guessed responsiveness on the basis of a story about hypnosis told by each subject during a test of imagination. A correlation of .34 ± .16 was found. The seven most hypnotizable subjects all stated explicitly in their stories that the imagined hypnosis was a success, while the remaining made the point incidental, merely implied it, expressed repugnance for hypnosis, or explicitly declared it a failure. If this subsequently discovered relationship be used as a criterion for a predictive rank-order the correlation is .56 or higher. The findings indicate both that subjects reveal their own attitudes in supposedly impersonal stories and that these attitudes affect the outcome of hypnotic trials.

In our experiments judgments of attitude were reached in a different manner than in White's study. For our study, three judges were asked to make a judgment of the subject's attitude toward hypnosis from the TAT protocols.

No amplification of this request was made. Each judge made his own judgment independently of the other judges. There was no contact between judges and subjects. No attempt was made to structure the attitude of the subjects prior to presenting the hypnosis card. The judgments were made on the following scale: very unfavorable, unfavorable, neutral, favorable, very favorable. The data from Table 2 and Table 4 show no relationship between these judgments and a prediction of hypnotizability. The percentage of agreement between judges is not high (Table 5). The uncontrolled variables in such a situation preclude valid judgments. Tested in this manner, TAT Card 12M does not qualify as an adequate predictor of hypnotizability.

The manner in which our judgments were obtained differed so much from White's that it is impossible to compare our data. It must be pointed out that the author's experiments were designed to test other hypotheses. The TAT data were accumulated as information subsidiary to the main study.

APPENDIX

Selected TAT Protocols

Each protocol is headed by symbols indicating judgments of attitude made and depths of hypnosis reached.

Symbols:

VF.....very favorable
F.....favorable
N.....neutral
U.....unfavorable
VU.....very unfavorable
R.....refractory
L.....light
M.....medium
D.....deep

TAT Protocols of Authoritarian Group

(In this group the subjects were told that they were going to be hypnotized.)

VU-VU-VU D

The man is the orphan girl's lawyer, who is trying to cheat her out of her father's estate. He is going to her in her sleep, awakening her and hypnotizing her. When she awakens she won't remember the incident, but she will obey his orders and make him the custodian of her estate. In this movie with eerie music a handsome young hero will enter and fall in love with the girl, and she with him. He will discover that the lawyer's intentions are detrimental to the girl's financial welfare and save the estate for her. The villain will be brought to justice and the young couple will live happily ever after on her money.

VF-VF-VF M

The person lying on the couch has come to a psychologist for treatment. He must have felt that the best way was to hypnotize her, for that is what he is doing. By hypnotizing her the psychologist can determine what is troubling her. After the patient wakes up, the psychologist will probably talk to her about her problems and try to straighten her out. They will discuss what the patient told the psychologist while under hypnosis.

VU-VU-U M

The picture shows two people; one, a man, lying on a bed or cot as if ill or dying. The other man appears to be either casting a spell or administering rites to the prone man. It appears as if one of the persons has come to the other one. For example, the "prone" person probably came to the other one if the other one is casting a spell, but if he is administering rites he probably came to the sick one. He is probably a priest of some sort. He is probably saying a chant or a prayer. The sick one is undoubtedly wondering if he is going to live, whether the priest can help him or not. I would say the sick one will probably die, although he will be put into "suspended animation."

N-F-F M

The picture, to me, contains a psychologist and his patient. The psychologist is trying to solve an important problem which concerns the patient's life. The patient is giving the psychologist a detailed rundown of a past incident which seems to have a direct bearing on the subject's difficulty. Previous to this, an incident has occurred to the patient which lingered in his unconscious mind and caused him an emotional disturbance. The psychologist will probably seek the reason for this and help the patient destroy the guilt he feels.

F-VF-VF R

I get the impression that the individual standing is a hypnotist and that the subject is on the bed. The individual on the bed

has troubles, and the hypnotist—or a better word to describe him would be psychoanalyst—is trying to bring out these problems by hypnosis. He is telling the individual on the bed to relax. The individual on the bed will become more enlightened when he comes out of the hypnosis by the explanation of the psychiatrist. He will find a solution to his problem.

N-U-U R

The people in this story are a minister and a dying man. The minister is praying or, in reality, offering a prayer for the dying man. The minister is thinking about the physical and mental status of the man. Will he survive? If not, what future has he in the great beyond? The minister is also thinking the flesh is only here for a short time, but the soul is eternal. The man is probably thinking, "I hope his prayers are answered. I hope, if not, I may continue to live happily ever after." Before this time the man has not given the future a preparation. But now time has elapsed or closed in on reality. Now he is preparing to go into another world.

VF-U-N R

The picture shows a hypnotist and his patient. The hypnotist has just put his patient to sleep. In another moment he will give his first command. The patient is not thinking, but the hypnotist is wondering what his first command will be. At the moment he is only saying some soothing words to quiet and relax the patient. After the hypnotist is finished with his patient, he will awaken her, and she will have no idea of what happened during the time she was under hypnosis.

TAT Protocols of Permissive Group

(In this group the subjects were told that they would be taught to hypnotize themselves.)

VF-F-VF L

(Who?) A patient (in the etymological sense) and a hypnotizer. (What?) They are trying to fool each other. The first subject thinks, "What is this cheat trying to prove?" The other: "For the first time in my life someone responds to my abracadabras." One says, "Concentrate, look at me with your eyes closed." The other, "That is something, isn't it?"

VF-F-VF L

This picture looks to me like a boy lying on a sofa. There is a man standing over the boy, who looks like he might be hypnotizing the boy. I receive this opinion from the way the man (or doctor) waves his hand over the boy's face. The boy seems to be sleeping and in full confidence of the man who is giving him the service. The picture looks to me as if there were no talking or thinking involved. I believe that the boy may be receiving a helpful service from the man.

U-F-N L

The two characters are a doctor with supposedly supernatural powers and a patient, who is the victim of some unknown disease. The doctor was called by the family of the sick man to perform a magic rite in order to secure the recovery of the sick man. He is not a doctor with a medical degree, but a sort of witch doctor. The rite is performed, and the sick man shows a remarkable recovery from the disease almost immediately. This is to these backward people a sign of magical powers.

VF-F-VF D

The woman is lying on the bed in a state of trance. The man has hypnotized her. He is in deep concentration. The man is holding his hand over her face. Neither of them is talking. He is kneeling on something. The woman's eyes are open.

U-U-N D

The people in the picture are two men. One is in bed, and the other is kneeling over the one in the bed. The man in the bed is sick, and the man kneeling over him is praying for him to get well. The man who is sick in bed is named John. John's father is praying over him to get him well. John thinks he will get well, and his father hopes his prayers will be answered. John has had rheumatic fever for eight weeks, and he is at the crucial stage, when one way he will die and another way he will live.

N-N-N R

It seems to be a boy and a girl talking about something. They are probably talking about something that has happened very recently. Previously they were probably on a picnic or some outing. Chances are he will probably take her home soon.

F-F-N R

The picture included a man and a woman. It seemed to me that the man was putting the woman into some sort of a

trance. Previously she might have just been lying there. The man is probably giving her some post-hypnotic suggestions. Later he will bring her out of the trance without her remembering what has happened.

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PERSONALITY FACTORS OF THE MMPI AND HYPNOTIZABILITY[1]

by Irving I. Secter, D.D.S., M.A.[2]

Many attempts have been made to relate refractoriness to hypnosis and certain personality traits. Attempts have also been made to find relationships between the degree of hypnotic susceptibility and personality factors. About this problem Weitzenhoffer (11) says:

> This is unquestionably one of the most ambiguous areas. A great many assertions have been made about the relation of personality traits to suggestibility, few of which have been substantiated in any satisfactory manner.

In a recent study Faw and Wilcox (6) concluded that susceptible hypnotic subjects have better personality adjustment than the insusceptible. In addition to the well adjusted there were a number of more poorly adjusted with high Hy (hysteria) scores on the Minnesota Multiphasic Personality Inventory (MMPI) who were susceptible. The insusceptible had higher MMPI scores on D (depression), Mf (masculinity-femininity), and Sc (schizophrenia) scales and had a poorer over-all adjustment.

White (14) posited that motivation contributes to hypnotic susceptibility. His recognition of the fact that many good subjects do not seem to be highly motivated led him to infer an "aptitude" for hypnosis. About this aptitude he stated: "It stands in a special relation to hysteria."

Sarbin (9) inferred a relationship between high Hy scores and hypnotic susceptibility. He gave the MMPI to 70 volunteers, who were college undergraduates. A hypnotic induction procedure resulted in 36 refractory subjects and 34 who were hypnotizable. Of the latter, 16 were categorized as somnambules and 14 were judged as light-trance subjects. Of the various scales on MMPI only the Hy scale differentiated the two groups. Sarbin wrote:

> Thus a scale which differentiates hysterical patients also differentiates hypnotic subjects. This finding recalls that part of Charcot's theory which regards hypnosis as artificially induced hysteria. However, none of the subjects was known to be a hysterical patient. We are led to the same conclusions made by clinicians for many years—the good hypnotic subject and the hysterical patient have something in common. We would suggest the role-taking attitude.[3]

Barber (1), in a study made on eighteen subjects, found significant rank-order correlations between the A (ascendance, .70), S (sociability, .56), E (emotional stability, .49), and O (objectivity, .47) factors of the Guilford-Zimmerman Temperament Survey and rank on hypnotizability. Confirmation of these findings would support the theory that hypnotizability in college students is related to desirable personality traits. Barber states:

> This study tends to confirm Baumgartner's (13) conclusion that suggestibility is a desirable trait; Friedlander and Sarbin's (9) report of a relation between "amiability" and hypnotizability; Barry's *et al.* (2), Wells' (13), and Friedlander and Sarbin's (7) studies indicating that hypnotizability is correlated more with "ascendance" than with "submission"; White's (14) study finding a high correlation between "extraversion" and hypnotizability; and Davis

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[1] This paper is part of a research thesis submitted to the faculty of the College of Arts and Sciences at Roosevelt University in candidacy for the degree of Master of Arts in the Department of Psychology. Donahue L. Tremaine, Ph.D., was chairman of the committee which read the thesis and was adviser for the project.

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[3] In role-taking the subject is presumed as behaving as would an actor in accordance with his concept of how a hypnotized person should behave.

and Husband's (5), Bartlett's (3), and Messer's *et al.* (8) conclusion that there is no relation between hypnotic susceptibility and neurotic tendency.

In a more recent study Weitzenhoffer (12), using 200 subjects, administered the Guilford-Zimmerman Temperament Survey and Cattell Sixteen Personality Factor Questionnaire. Hypnosis was induced. Pearson correlations were computed for 15 of the Cattell factors and nine of the Guilford-Zimmerman factors and hypnotizability. No significant correlations were found.

Secter (10) administered the MMPI to 48 subjects with a history of previous refractoriness to hypnosis. Induction procedures were applied. Distribution of hypnotic depth is shown in Table 1.

Analysis of variance was used to determine whether subjects classified as refractory or entering light, medium, or deep levels of hypnosis differed in mean scores on the MMPI scales. Table 2 summarizes the results. As can be seen from the Table, in no case was there a significant difference between MMPI scores for the several levels of hypnosis.

An item analysis was made on the statements which compose the MMPI to determine whether the response to any of the items was related to hypnotizability. The numbers of "true" and "false" responses made in each of the four depth categories were compared for each of the 566 items of the MMPI. Differences in depth of hypnosis between those who answered "true" and those who answered "false" to the

TABLE 1

DISTRIBUTION OF HYPNOTIC DEPTH FOR 48 SUBJECTS

Hypnotic depth	Refractory	Light	Medium	Deep	Total
Number of subjects.....	6	11	23	8	48

TABLE 2

COMPARISON OF MEAN MMPI SCORES FOR SUBJECTS ENTERING VARIOUS HYPNOTIC LEVELS

MMPI Scale	F	Group Means**				Grand Mean
		R	L	M	D	
Hs	1.22	50.33	48.36	52.73	48.25	50.64
Hs*	1.14	53.00	50.91	54.05	47.25	52.06
D	.70	57.50	54.27	58.00	51.63	55.98
Hy	1.35	50.50	53.27	55.05	49.38	53.09
Pd	.46	51.50	52.91	52.36	47.63	51.57
Pd*	.70	54.83	55.36	54.27	48.50	53.62
Mf	.04	61.17	61.09	61.36	60.13	61.06
Pa	.68	54.50	49.09	53.18	50.75	51.98
Pt	.24	50.83	49.55	50.82	52.25	50.77
Pt*	.29	55.17	53.09	51.91	52.00	52.62
Ma	.87	54.83	61.64	57.23	58.88	58.23
Ma*	1.17	55.50	63.64	58.14	59.38	59.30
Sc	.21	48.83	50.45	49.18	50.88	49.72
Sc*	.61	52.83	55.36	51.45	50.88	52.45

*Corrected for K.

**R, L, M, D are the depths of hypnosis: refractory, light, medium and deep. Degrees of freedom in all rows above: 3/43. P in all rows is greater than .05.

MMPI items were analyzed by a chi-square test. The refractory and light categories were combined and the medium and deep categories were combined to insure a sufficiently large expected value for each cell. As shown in Table 3, there were statistically significant differences for only seven items. Responses of "true" to items 20, 230, 435 and 450 were related to refractoriness. Responses of "true" to items 180, 442, and 483 were related to hypnotizability. Such differences could have resulted by chance alone, and it is therefore unlikely that these differences are in any way meaningful.

The MMPI data were further analyzed, using the Harris sub-scales and the factor scales derived by Harris. No significant differences were found.

TABLE 3

MMPI Items Showing Statistical Significance on Item Analysis***

MMPI Item	df	Chi-square
#20	1	4.05*
#180	1	5.11*
#230	1	2.96**
#435	1	6.32*
#442	1	3.45**
#450	1	4.84*
#483	1	5.65*

*Significant at 5% level.

**Significant at 10% confidence level.

***Yates' correction for continuity was used in computing chi-square.

MMPI Items Showing Significance

20: My sex life is satisfactory.

180: I find it hard to make talk when I meet new people.

230: I hardly ever notice my heart pounding and I am seldom short of breath.

435: Usually I would prefer to work with women.

442: I have had periods in which I have lost sleep over worry.

450: I enjoy the excitement of a crowd.

483: Christ performed miracles such as changing water into wine.

DISCUSSION

Faw and Wilcox (6) found higher MMPI scores on the D, Mf, and Sc scales for the insusceptible. Data from Table 2 indicate no significant relationship between any of the MMPI scales and either refractoriness or hypnotizability. The susceptible subjects showed even higher, but not statistically significant, scores on D, Mf, and Sc scales than did the refractory. Faw and Wilcox made their observations on college students, whereas our observations were made on mature professional men with a higher mean age and greater age range.

The Hy factor has sometimes been associated with susceptibility. Table 2 shows a higher, but not statistically significant, Hy score for the refractory than for those who entered the deep stage. These results are in line with Weitzenhoffer's statement (11), who asserts that the data on personality show no definite pattern.

It must be taken into account that the subjects of our experiments constituted a highly biased sample consisting of mature professional men (primarily physicians and dentists).

CONCLUSIONS

Comparisons of the above findings with those of other reports cannot be undertaken until it can be established that susceptibility was measured in each investigation in ways which make it possible to compare results.

For the population tested in this study MMPI scales or answers to MMPI items do not seem to be significantly related to hypnotizability. The use of such scales or items to predict hypnotizability would not be justified on the basis of data from Table 2 or Table 3.

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SOME IMPLICATIONS FOR PERSONALITY THEORY AND FOR PSYCHOTHERAPY BASED ON RESEARCH EMPLOYING HYPNOSIS

by Arthur Z. Orzeck, Ph.D.[1]

In 1958 an article by Orzeck, McGuire, and Longenecker (5) appeared under the title "Multiple self concepts as effected by mood states." The essence of the article is as follows:

"An experiment was performed to study the hypothesis that two distinct selves occur when moods are varied. Following a definition of the term 'self' an experiment was designed to produce hypnotically two distinct self concepts by the induction of the moods of depression and elation. Twenty *S*s responded to an identical self-reference scale 3 times, first during the waking state, then under hypnotic depression, followed by hypnotic elation. Memory was controlled hypnotically. The scale consisted of 100 items evenly divided for adjustment and maladjustment. Chi-square analyses for each *S* revealed that 14 *S*s apparently shifted orientation, while 6 *S*s did not. It was speculated that there was considerable similarity between these artificially induced value orientations and actual case recordings."

While this study was initially designed to confirm a feeling that actual case recordings of multiple personality were but selected cases of a common phenomenon that occurs in much disguised form, since the completion of the experiment another significant implication has been derived. The experiment also showed that cognitive self concepts were the product, at least in part, of the moods of the individual. When the individual was depressed, his values were negative. When he was elated, his values were positive. Not part of the intended suggestion was the quality of the depression and elation induced. When suggestions to feel depressed were made, the subjects evidently interpreted this to mean that they should think negatively. When the suggestions to feel elated were made, the subjects interpreted this to mean that they should feel happy and cheerful, but not euphoric, which might have reflected a different kind of "positive" feeling. The difference specified will be discussed again later. The new implications suggest certain considerations for theory of personality and therapy.

Any theory of therapy, whether group or individual, should issue from a theory of personality. If a completely coherent, well-rounded theory of personality were the prerequisite, we might still be waiting for therapy, and most therapists would argue that something much less than a complete theory of personality is quite adequate for the conducting of therapy. Every therapist operates with some personality theory, and in most cases it is an eclectic affair. It is not clear that all therapists work with a prearranged plan. What they do is simply employ little bits of techniques, an understanding of dynamics which they are very ready to use and with an almost preconscious awareness that, whatever they do or say to the patient, they must always give the impression that they know what they are doing. It is my feeling that most therapists set very narrow limits in therapy to avoid becoming too familiar with the patient and by keeping the distance safe they maintain an illusion that they are in command. Familiarity, they feel, is not conducive to treatment. The result is that the patient always wonders about the therapist, and his fantasies

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usually make him an authority figure. The therapist makes an occasional knowing remark, and the patient permits himself to play the dependent role.

Models of the psychoanalytic and the client-centered therapies are widely known. Most therapists borrow from them but few depend on them exclusively. Part of the therapist's eclectic approach is the contribution of his own flavor of therapy. Regardless of the special ingredients, each therapist must answer for himself whether or not insight into problem areas is the primary means or goal of therapy. If he answers "yes," this is the meaning of therapy, he must explain how insight helps, and he must explain why it obviously doesn't help sometimes. His answer to the latter usually amounts to a differentiation between intellectual and emotional insight. The argument seems good at first glance. How does emotional insight take place? First, we know it takes time. It is not achieved very quickly. A patient may come close to saying what his difficulties are, but the therapist usually waits a long time (perhaps a year or more) before he makes an interpretation that leads to emotional insight. He argues that if the interpretation were made early it would have only intellectual meaning and would be of little value. Emotional insight takes time.

Another therapist might argue that it is not insight that he deals with so much as it is the building of ego strength. He explains that emotional insight takes time because it takes time to build up the ego. When the ego is sufficiently strong to tolerate the insight, then the insight occurs emotionally. Before the ego is strong enough, only intellectual insight, if at all, is possible. This latter therapist might then argue that insight is not even necessary, since the ego is strong and the insight does not make it any stronger. The first therapist argues it is the insight that rebuilds the ego, while the second therapist argues it is the strengthened ego that permits the insight. Since it takes quite a time for the insight to occur (a year or so), the second therapist seems to have a better argument. What has been taking place during the year, if not emotional insight? Certainly something was happening, and it is superficially easier to accept that some ego building was going on. How did this ego building come about? Though the first therapist did not plan for it, he was an accepting person, a knowing person, a respected and respecting person. He gave the patient a good relationship, and the patient borrowed on the therapist's security and strength. Now just what did the therapist do specifically that produced in the patient a stronger ego? During the year, he certainly made interpretations of a milder sort. He listened intently to what the patient said. He treated the patient with intrinsic respect. He psychologically held the patient's hand when the patient was very upset. From this, two kinds of operation can be seen.

The building of ego is a two-sided affair. The individual is provided with a feeling of self-worth through the responses of the therapist who accepts him as worthy, and the patient's ego also grows by his gradual increasing ability to withstand his own and the emotional stresses from others. There is much to the argument that self-worth can only be produced by this increasing ability to tolerate stress together with (and impossible without) the direct feeling that the self is worthy and able to handle stress or problems of living constructively. The argument extended here is that a good therapist, regardless of whether or not he depends upon insight, gives his patient a feeling of self-worth and ability to tolerate stress. If he produces in the patient some valuable insights, this

is of some help, but may, in the last analysis, be a form of luxury. Therapy cannot hang on insight as a necessity, since many patients seem unable to engage in this kind of relationship. It may well be that for some people part of their self-picture includes knowing themselves and so therapy must include insight. With this insight and a stronger ego, they feel self-respecting. It is advisable to remember what William James said about the "sentiment of rationality" (3), for it is easy to feel satisfied when you know the "truth" though a better set of facts elsewhere would indicate that your "truth" is illusory. One tends to feel better or "freer" because he knows the "truth", but the feeling is but a sentiment. Insights could be very false, and if a patient acquires these insights from a biased therapist, his "truth" can only be such a sentiment. If he feels better, he attributes it to his newly found insights, his new values, and it is not strange to see that his therapist approves of his acceptance of these insights. As a matter of fact, the therapist himself holds similar values. This is the patient's confirmation.

It is needless to indicate that the thesis of this paper is that insight is almost irrelevant. The unsophisticated healthy-minded man who never goes to a therapist has little more than superficial insight. Insight is the product of the disturbed individual's search for salvation, and his insights are satisfying if he is an early Greek philosopher, a medieval theological apologizer, a modern positivist, or a Freudian analyst. Everybody's insights are different, yet everybody is made comfortable by his own brand. He should be troubled when he looks around and finds people "thoroughly misled" acting in a confident manner. And from a "strictly scientific" point of view it is not hard to find extreme cases of unfactual belief and there to find someone functioning very well in society, doing good works for himself, his family, and others by *scientific standards*. An opposite kind of parallel may be found in the case of an unscientific believer who looks at a particular scientist with amazement, for he sees the scientist living the "good life" in spite of his scientific beliefs. It is almost as if all behavior were a *non sequitur* of personal belief. An example of the *non sequitur* relationship of belief and behavior should be cited before the discourse moves on. It was popular (and still is in some quarters) to employ logical arguments for the existence of God. There is the teleological argument that there must be a purpose to existence, and so God must be a reality. Then there is the cosmological argument that the universe had to be started and only God could do such a grand thing. And the ontological argument runs that thought of God could not have occurred to man unless God planted the thought because he did in fact exist. Immanuel Kant (4) reduced them all to a single argument, the ontological argument, when he pointed out that an idea of purpose, cause, or existence were all variations on the ontological argument, and he destroyed this argument by pointing out that to have an idea of anything is not identical with its paralleled existence. While this is dramatic enough, the most remarkable aspect of all this argumentation is something again. Suppose a particular theologian has not read Kant and does not know of these errors. He can still argue that God exists and feel a certain sentiment in his own rationality. But it certainly is a *non sequitur* to argue that his own brand of religion issues from any or all of the former arguments for the existence of God. Each man's brand of religion includes beliefs, some of which result in behavior, and none of these behaviors can be shown to follow necessarily or logically from his primary argument for the existence of God.

All this is an argument by analogy. The stage is set to entertain the possibility that insights into one's personality do not necessarily result in truth or in directives for living a better life. And yet such a position is tricky, for it suggests that no directives can be given or that directives are quite senseless in the last analysis. This possibility can be entertained though the thought is somewhat disturbing.

If there is a relationship between values as directives and behavior which should be consistent with psychological health on the one hand and with ill health on the other, the relationship should be found. Many men have tried to present certain ideas as the beneficial ideas and certain other ideas as the harmful ones. Very many disagreements exist, and it is proposed here that no particular comprehensive set of directives or valuations can ever be correct. If this were the case, all people would be as carbon copies. All people would be easily predictable if their behavior followed from their beliefs; we know this is not true. If it is a mistake to pursue the truth in order to be freed, what should man pursue? Obviously the alternative (which would be the pursuit of falsity) is not reasonable. It must be that there are *kinds* of truths and man must seek that kind of truth which is independent of cultural or accidental value systems.

An analysis of samples of healthy-minded people might lead to the kind of truth man should seek to become mentally healthy. The hypothetical healthy man has no logically consistent belief-behavior system. He may have some consistencies between belief and behavior, but an outsider may note that fortunately his "false" beliefs do not prevent him from acting in a healthy style. Fortunately for our misinformed citizen, he is comfortable with himself and others and can tolerate adversity without blaming others, himself, or life in general. While false beliefs can act as neurotic defenses, for our healthy-minded citizen they do not. His belief system is a fantasy, a sentiment, a luxury, a justification for healthy living, but whenever the belief system opposes healthy living, he ignores his system and lives in the real world. The difference between him and his neurotic brother is that the latter uses his personal system of belief, his fantasy, his sentiment, as a defense against reality.

Such a conclusion suggests that each personality consists of two orientations, one of reality, the other of fantasy (or belief system, "insights", what-have-you). In reality, the individual is worthy, just as good as any one else. He also can tolerate stress consistent with his biological and psychological endowment. He engages in life naturally; that is to say, he lives by certain *empirical imperatives*. He eats, sleeps, seeks protection from danger, loves, walks erect among his fellows, and is curious. He appreciates the esthetics of living and is secure in his ethical expectancies. If a man lives in such a manner he is healthy and his value system may be varied and unique. His only imperative directives, which require no special system of learning, are innate. Though he comes into life with a built-in innate system, he can have this system undermined by an external prevention of this reality system. If the reality system is too well prevented, he has no choice but to act on the unreality system.

Certain values are postulated as innate, absolute, uncompromising beyond a certain limit. None of these values cited are the kind that pursuers of knowledge suggest will make a man free. Mostly the kind they advocate produce only a sentiment, a fantasy of freedom. The only kind of value system that can be relied on are those values which are compelling for man *qua*

man—regardless of culture and specific learning.

Max Eastman in his *Enjoyment of Laughter* (1) pointed out that humor is only possible if a man feels like laughing. No joke can come off if the person aimed at feels poorly. As a matter of fact, he feels even worse if he is not in the *mood* for humor. While Eastman worked only with humor, he set the stage for a consideration of all human moods. Students of child development mainly agree (I believe) that smiling on the part of the baby is maturational and not learned. The argument, consistent with them and Eastman, is that all moods are innate. The appropriate stimulus produces a smile, anger, anxiety, all in due maturational time. Different stimuli may come to provoke these moods, but the substrata of the mood are maturational. Although only briefly experimental (see author's study above), there is evidence to believe that a happy (not a euphoric) man is healthy-minded and will treat himself and other men in an equitable manner. A hostile (not an assertive or reactively angry) man treats himself and others poorly. This is to argue that certain kinds of emotion are healthy and certain kinds are not, and it is the fostering of good emotion which makes for healthy-mindedness regardless of specific learned personality styles. For every healthy mood there is the opposite unhealthy mood. They can be listed in part, as follows:

Healthy	*Unhealthy*
Happiness	Euphoria
Assertiveness, reactive anger	Hostility
Sadness	Depression
Fear (reactive anxiety)	Anxiety

Others perhaps could be listed, but these are probably the principal moods. The healthy and unhealthy moods look alike in some respects, but the words chosen suggest something which leads to a more definitive difference. The unhealthy terms look phony as moods because reality is missing. That aspect of reality which is missing is personal self-worth. The euphoric, depressed, hostile or anxious person feels personally self-debased. The happy, assertive, sad, or fearing person does not whip his ego. Freud (2) pointed out the difference in his "Mourning and Melancholia." Further clues were gleaned from Eastman, and experimental evidence for these distinctions exist in the study cited above.

A beginning of a personality theory is now apparent. Each personality consists of certain innate moods which are expressable in a healthy or unhealthy manner. In the healthy manner personal self-worth does not diminish, whereas in the unhealthy manner the self-worth is diminished. With this beginning, therapy then can take a definite form. The object of therapy is to foster those moods which are healthy, undefensive, and the discouragement of those moods which are unhealthy, defensive. The object is to build ego, to destroy ego defensives; the latter is done not by direct attack or interpretive analysis but by disuse. With the fostering of healthy moods, the unhealthy moods fall into disuse. The therapist produces these good moods by active participation in these moods. *He displays these moods to his patient.* He permits his patient to see him as a *healthy-mooded* person. He does one more thing which makes it difficult to see what he is *actually* doing. He conducts therapy in terms of his patient's expectations, to some extent. If the patient is a talker, he talks with him and he talks perhaps in the patient's style. If the patient is mainly silent, he also talks but he does it in a quiet manner. The therapist employs many *vehicles* for communication of the healthy moods. If the vehicle is insightful exploration of one's psyche, he does this. If the vehicle is the pursuit of special knowl-

edge, he does this too. He may likely try to structure the vehicle to fit in with his own special abilities, since he cannot be verbal at all levels. By helping to structure the vehicle to that of the insight type, he can employ many subtleties in a show of ableness and he can thus persuade his patient that he is a self-respecting person quite able to withstand the stresses of living. Such a structuring is almost a necessity if he is to give his patient a picture of personal self-worth which he, the therapist, wants to communicate eventually to the patient. He puts himself in a place of special advantage, and by so doing he can be everything he wants his patient to become.

The therapist does one more thing which is a by-product of the fostering of healthy moods. The nature of healthy-moodness is that they do not persist beyond the limits of the demands of reality. What are the demands of reality? This is certainly difficult to specify, but they can be estimated by comparing emotional reactions that are more apparently unhealthy. Unhealthy moods (or emotions) tend to linger excessively. A bereaved person may still be depressed years after a beloved one dies. Or a hostile man is still hostile three weeks after he gets a traffic ticket. A husband is a sore-head for weeks after a small argument with his wife. In the extremes the differences are clear. All the above examples are of moods which supposedly are reactions to real stress or provocation. The difference between them and healthy moods is that they linger beyond warrant, and they do this because they include the condition of self-devaluation. Healthy-mooded people might have the same experiences and would recover relatively quickly. One more point should be made. No argument is here made that healthy or unhealthy moods should be in response specifically to appropriately healthy or unhealthy provocation. That is, one might see a healthy-mooded person becoming angry in a situation that does not warrant the anger. A second look would show that the situation was not simple but had a certain double meaning which produced little justifiable anger — but if the person is essentially healthy-mooded, he will recover from the anger quickly. The object of this example is to suggest that nobody, healthy or unhealthy, can be expected to respond emotionally only to real provocation. All too often the healthy-mooded person reacts unrealistically, but he, unlike his neurotic brother, quickly recovers and returns to reality and adequate responses to reality.

SUMMARY AND RECOMMENDATIONS FOR FURTHER RESEARCH

The thesis stated here is derived from an experiment which employed hypnosis. In the experiment subjects were hypnotized and were given suggested moods, during which time they responded to identical self-reference statements. When they were happy they held positive values, and when they were depressed, they held negative values. No attempt was made to persuade them to think positively or negatively with respect to specific values. Their values shifted because their moods shifted, and it was therefore hypothesized that the essential aspect of personality theory should be the moods or emotions of people and not their specific belief-behavior system. From such a beginning theory a system of psychotherapy was proposed that would emphasize the individual's moods and would strive to bring about a positive or healthy mood-system. The special ingredient differentiating healthy from unhealthy moods is the ingredient of personal self-worth. With an ego or self that feels respected, the moods change their quality. Euphoria becomes happiness, depression becomes sadness, hostility becomes anger, and anxiety becomes

fear. All of these healthy emotions are then employed in response to reality in the main, but in all cases they do not linger beyond the demands of reality.

An experiment comes to mind to bear out the importance of mood over insight. The experiment, employing hypnosis, could show the dimensions of normal kinds of psychotherapy in capsule form. Let four equated (normal) people be hypnotized. Let a single therapist induce identical neurotic problems for each subject. Let the therapist work out the problems in the following hour outside of the trance situation. Since the problem is exactly the same, each person's "dynamics" would be the same, and the insights produced would be the same. Let one therapy occur with the therapist expressing healthy moods with the fostering of insight; let another therapy occur with the therapist expressing an anxious-hostile approach while also fostering insight; let a third therapy occur with the therapist expressing healthy moods without fostering insight; and, finally, let the fourth therapy occur with unhealthy moods and no fostering of insight. All four possibilities would then be observed. In the case of those therapies involving insight, insight could be guaranteed by post-hypnotic suggestion. With the two involving no insight, nothing would be suggested beforehand about a resolution by insight (but a provision could be arranged to provide insight if necessary after the experiment is over). Three to five judges from psychology and psychiatry could evaluate the outcome of the four therapies. They might be rated to compare one with the others. It is hypothesized that the moods will determine the quality of the recovery rather than the insights produced. This research is already planned and will be carried out in the near future.

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HISTORICAL NOTE ON THE HAND LEVITATION AND OTHER IDEOMOTOR TECHNIQUES[1]

by Milton H. Erickson, M.D.[2]

In the spring of 1923 at the University of Wisconsin, interest was expressed by Clark L. Hull, Ph.D., Associate Professor of Psychology, in the writer's experimental work on hypnosis. The suggestion was offered that the writer continue his studies throughout the summer and then report upon them before a post-graduate seminar on hypnosis to be conducted by the Psychology Department.

All of this was done, and the first formal post-graduate course in hypnosis was initiated at the University of Wisconsin in September 1923, probably the first one in the United States. This seminar was devoted to a systematic examination and discussion of the summer's experimental procedures and findings reported upon or demonstrated before the group. Also presented was additional work initiated and performed by the writer during that academic year.

During that summer of 1923, among other things, the writer became interested in automatic writing, first secured from subjects in a trance state and subsequently by post-hypnotic suggestion. This gave rise to the possibility of using suggestions conducive to automatic writing as an indirect technique of trance induction for naive subjects. Although successful, it proved to be too slow and laborious an induction technique in most instances. It was modified by suggesting to the subject that, instead of writing, the pencil point would merely move up and down on the paper, or from side to side. The vertical or horizontal lines thus secured were later found to be an excellent approach to the teaching of automatic writing to difficult subjects.

Almost from the first trial it was recognized that the pencil and paper were superfluous and that the ideomotor activity was the primary consideration. Accordingly, the writer, using his younger sister Bertha as a subject for the first time, induced a somnambulistic trance by a simple hand-levitation technique. Thereafter many variations of this original technique were devised until it became apparent that the effectiveness of many supposedly different techniques of trance induction derived only from a basic use of ideomotor activity, rather than from variations of procedure, as is sometimes naively believed and reported. Perhaps of all the many variations of ideomotor techniques of induction that may be devised, the more generally useful are (1) simple direct hand-levitation because of the possibility of visual participation, and (2) the slightly more complex rhythmical hand levitation, in which visual and memory participation frequently lead to the ideosensory response of auditory hallucinations of music and the development of a somnambulistic trance.

Another highly technical and complicated procedure of trance induction was developed that summer and repeated in many variations, but with no real understanding at that time of what was involved. A sixteen-year-old boy, who regularly drove a milk wagon, had never before been hypnotized. He was asked to sit quietly in a chair and silently review in his own mind every feeling throughout his body as he systematically recalled the events of the twenty-mile milk route over which he regularly drove a team of

[1] Also being published simultaneously in Spanish in La Revista Latino-Americana de Hipnosis Clinica, issued in Argentina by La Sociedad Argentina de Hipnoterapía.

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horses. The further explanation was given that, even as one can remember names, places, things, and events, so could one remember body feelings of all sorts and kinds. This he was to do by sitting quietly in the chair with his eyes closed and to imagine himself driving along the highway, feeling the reins in his hands and the motions of the wagon and of the wagon seat.

Shortly it was noticed that he was shifting his hands and body in a manner suggestive of the actual experience of driving a team of horses. Suddenly he braced his feet, leaned backward, and presented the appearance of pulling hard on the reins. Immediately he was asked, "What are you doing now?" His reply was, as he opened his eyes, "Going down Coleman hill." (The writer himself had often driven that same milk route in the same wagon and recognized the characteristic behavior of handling the team in going down that steep tortuous hill!)

Thereafter, with his eyes open and obviously in a somnambulistic trance, although he continued to sit in the chair, the boy went through a long, slow process of seemingly driving the horses, turning now right, now left, and heaving with his shoulders as if lifting cans of milk, thus reliving largely the experience of actually driving the milk route. The writer's own experience with that same milk route permitted a ready recognition of the progress being made along the route.

However, at one particular stretch of the road where there were no farm houses, the boy went through the motions of pulling on the reins and calling "Whoa!" He was told to "drive on" and replied "Can't." After many futile efforts to induce him to continue driving and always eliciting the same response of "Can't," he was asked why he couldn't. The laconic reply of "Geese" was given. The writer immediately recalled that on infrequent occasions in his own experience a certain flock of geese happened to choose the moment of the milk wagon's arrival to cross the highway in single file on their way to another pond, thus stopping traffic.

This first trance lasted several hours as the boy went through the events of the "trip," and it seemed impossible to break into and interrupt it. Not until he turned the horses into the home driveway could the trance be terminated.

This particular trip was repeated in later similarly induced trances with similar results. The boy was also asked to relive other trips, in none of which the geese happened to appear, but his neglect of the established practice of letting the horses rest at a certain customary spot was disclosed in one such reliving.

At the time of this work, there was no recognition by the writer of kinesthetic memories and images as a trance induction technique, but it led to a systematic and profitable investigation of the possibility of using any sensory modality as a basic process in inducing hypnotic trances.

During his first demonstration of the hand levitation technique of trance induction to that 1923-1924 seminar group, a special finding was made by the writer of the spontaneous manifestation in a volunteer subject of hallucinated ideomotor activity. She had volunteered to act as a subject for a demonstration of what the writer meant by a "hand levitation trance induction." While she and the group intently watched her hands as they rested on her lap, the writer offered repeated insistent appropriate suggestions for right hand levitation, all without avail. Silent study of the subject in an effort to appraise the failure of response disclosed her gaze to be directed into mid air at shoulder level, and her facial expression and apparent complete detachment from her surroundings indicated that a deep trance state had developed.

She was told to elevate her left hand voluntarily to the level of her right hand. Without any alteration of the direction of her gaze, she brought her left hand up to shoulder level. She was told to replace her left hand in her lap and then to watch her right hand "slowly descend" to her lap. When it reached her lap, she was to give immediately a full verbal report upon her experience. There resulted a slow downward shifting of her gaze and, as it reached her lap, she looked up at the group and delightedly gave an extensive description of the "sensations" of her hallucinatory experience, with no realization that she had actually developed her first known trance state, but with an amnesia for the reality of the trance experience as such, though not for the content.

She asked to be allowed to repeat her experience and promptly did so. This time the group watched her eye and facial behavior. Again, there was no hand movement, but all agreed that she developed a somnambulistic trance immediately upon beginning to shift her gaze upward. This conclusion was put to test at once by demonstrating with her the phenomena of deep hypnosis. She was then aroused, and there followed an extensive discussion of "kinesthetic imagery" or "kinesthetic memories" as possible techniques of hypnotic induction. The writer was assigned the task of further experimental work on these ideas, to be reported at the next meeting.

That report, in brief, was simply that trances could be induced in both naive or experienced subjects by techniques based upon (1) the visualization of a motor activity such as hand levitation or by visualizing the self climbing up or down a long stairway, and (2) upon "remembering the body and muscle and joint feeling and sensations" of motor activity of many kinds. To this was added the report on the findings with the sixteen-year-old boy.

Approximately 15 years after these earlier studies on ideomotor techniques had been reported to the seminar group at the University of Wisconsin, another study was begun. This was initiated by the observation that, especially at lectures on controversial topics, there are those in the audience who will unconsciously slowly nod or shake their heads in agreement or disagreement with the lecturer. This observation was further enhanced by noting that certain patients, while explaining their problems, will unwittingly nod or shake their heads contradictorily to their actual verbalizations. These informative manifestations suggested the possibility of utilizing this type of ideomotor activity as an hypnotic technique, particularly for resistant or difficult subjects, although it can also be used readily on naive subjects.

The actual technique is relatively simple. The explanation is offered to the subject that an affirmative or a negative answer can be given by a simple nod or shake of the head. Also, it is explained that thinking can be done separately and independently by both the conscious and unconscious mind, but that such thinking need not necessarily be in agreement. This is followed by asking some question phrased to require an answer independent of what the subject may be thinking consciously. Such a question is, "Does your unconscious mind think you will learn to go into a trance?" After being asked this type of question, the subject is told to await patiently and passively the answering head movement which will constitute the answer of his "unconscious mind." A rapid or forceful response signifies a "conscious mind" reply. A slow, gentle head movement, sometimes not perceived by the subject, constitutes a di-

rect communication from the "unconscious mind." With the response, catalepsy develops and a trance state ensues rapidly.

Or, as a simple variation, one can suggest that the levitation of one hand signifies the answer "yes," the levitation of the other, "no," the levitation of both, "I don't know" and then ask the above or a comparable question. The development of a trance state is concurrent with the development of levitation, regardless of the significance of the reply.

These techniques are of particular value with patients who want hypnosis, who could benefit from it, but who resist any formal or overt effort at trance induction and who need to have their obstructive resistances by-passed. The essential consideration in the use of ideomotor techniques lies not in their elaborateness or novelty but simply in the initiation of motor activity, either real or hallucinated, as a means of fixating and focussing the subject's attention upon inner experiential learnings and capabilities.

BRIEF CLINICAL REPORTS

TRANSURETHRAL RESECTION OF CARCINOMA OF BLADDER UNDER HYPNOSIS: A CASE REPORT

by Victor S. Lait, M.D.[1]

Hypnosis has been frequently used to facilitate cystoscopy. The following case illustrates an extension of this use.

P.P., a 49-year-old man, was seen by the author complaining of sudden, painless hematuria. There was a history of two previous similar episodes in the past two years. Cystoscopy was performed under hypnoanesthesia, which was achieved rapidly by the hand transfer method, the bladder was irrigated, and a growth was visualized near the meatus of the left ureter. It was decided to attempt transurethral resection the following week.

The night before surgery there was another episode of bleeding with acute urinary retention due to a clot, and catheterization was required. A 5 cc. soft rubber retention catheter was introduced into the urethra before it was discovered that the stylet had been mislaid and it was impossible to pass the sphincter without it. The patient was instructed to go into trance and relax; as he did so all resistance suddenly disappeared, and the catheter was introduced into the bladder with ease.

One hour preoperatively 100 mgm. Demerol were administered intramuscularly. The patient was hypnotized on the table, and hypnoanesthesia was again achieved by the hand transfer method. After cystoscopy and inspection and irrigation of the bladder, introduction of the transurethroscope was attempted by the urologist. At this time the patient complained of a feeling of suprapubic pressure. He was instructed to further anesthesize the area by hand transfer, and he succeeded in doing so in one minute. Instrumentation then proceeded, and resection of a Grade I-II carcinoma of the bladder was accomplished with the electric knife without further discomfort. The patient and the author carried on a conversation on extraneous matters during the entire operation, which took 1½ hours. The post-operative course was smooth and uneventful.

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EFFECT OF HYPNOSIS ON EDEMA: A CASE REPORT

by Victor S. Lait, M.D.[1]

Vascular changes characterized by blanching of the skin or flushing and sweating are frequently observed during hypnoanesthesia or relaxation under hypnosis. The following case illustrates the practical utilization of one of these phenomena.

J.O., an 83-year-old woman, was first seen by the author 24 hours following a fall. She exhibited two hemorrhagic blebs, each ¾ inches in diameter on the medial aspect of the dorsum of the right foot surrounded by an area of edema and erythema 3 inches in diameter. There were hematomas of the base of all toes. The edematous area was exquisitely tender. In addition, there were present atrophic changes of the skin of both ankles characterized by scaling and pigmentation, and there were large dry, scaly patches of dermatitis on the calves, thighs, and arms which demonstrated many artifacts due to scratching.

There was a history of recent generalized purpura due to drug sensitivity. This had been attributed to medication prescribed for the treatment of a patchy, pruritic exfoliative lesion involving many parts of the body, which had been diagnosed as eczema. There was a history of skin sensitivity to many local applications. A dermatologist had interdicted all further medication, and the purpura had disappeared.

Discussion by the author with the consultant elicited the advice that no medication be prescribed, and that with the possible exception of a weak Aveeno colloidal

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solution, no local applications be used. This advice was followed, and the patient was advised to practice Buerger's exercises. Twenty-four hours later the erythema and edema had spread, involving the entire dorsum of the foot. The patient complained of sleeplessness due to pain and throbbing. The wet dressings were discontinued, and after the patient had spent another sleepless night with no regression of the symptoms, it was decided to attempt hypnosis for relief.

Since the patient was somewhat childlike, the following verbalization was used. "That hurts a lot, doesn't it? . . . . And it might hurt a lot worse, mightn't it? . . . . It would be nice if it would hurt less, wouldn't it? . . . . Perhaps it would hurt less if you relaxed; would you like to try?" Trance was then achieved in five minutes by the usual progressive relaxation technic, and the patient agreed that she felt comfortable and drowsy enough so that it would be easy for her to sleep. It was then suggested that she permit the muscles and blood vessels in her legs to continue to relax after she "roused up" and that she use what she had learned at any time she so desired and particularly before retiring in order to achieve the comfortable, drowsy feeling.

When seen two days later she stated she had had no pain since the first trance, had followed instructions, and had slept 11 hours each night. The skin of the foot was no longer tense and shiny, the erythema and hematoma were fading, and the edema had receded considerably over the area of the 5th metatarsal. It was noticed that the patient was still scratching the lesions on legs and arms. Trance was induced, and after reinforcing previous instructions, it was suggested that the same feeling of comfort could be developed in the areas of the rash so that it would not be necessary to scratch as much.

These suggestions were followed, and the patient continued making progress so rapidly that a week later she attempted the stairs alone, slipped and fell, and abraded one arm severely. In spite of a history of sensitivity to merthiolate, trance was induced and merthiolate was applied while suggestions were offered that the patient pay no attention to what was done so that it would not distress her in the future. Her recovery was uneventful.

ACUTE URINARY RETENTION SUCCESSFULLY TREATED WITH HYPNOSIS

by S. Cucinotta, M.D.[1]

Mrs. C., age 37, was admited to the hospital with a chief complaint of hematuria. On the same day that she was admitted she developed urinary retention requiring catheterization.

She was cystoscoped on the following day and was noted to have inflammation in the region of the trigone. The remaining bladder area was normal. The urethral openings were normal. A No. 5 urethral catheter was passed with no obstruction to the kidney pelvis. Culture and sensitivity tests were taken, and 5 cc. of 10% argyrol was placed in the bladder.

Following cystoscopy, the patient was unable to void for the remaining part of the day and required catheterization.

Urecholine mgm. 20 three times a day was used on the third day. This measure failed to start the urinary flow. On the fourth day an indwelling catheter was used and was permitted to remain for an additional three days. This use of an indwelling catheter was not effective. Continued reassurance was given to the patient that she had an acute infection and that she would be able to void as soon as the inflammation subsided. There was no local pain within the bladder to cause her to fear to void.

A consultant was called for an opinion and further recommendations. He cystoscoped her again and reported that there was only a very slight inflammatory area at the site of the trigone, and he could not now find pathology to account for this urinary retention. He recommended that the indwelling catheter be replaced and, if this did not relieve her condition, then to consider spinal cord involvement because no objective pathology was noted.

On the eleventh day, following removal of the inlying catheter, she still was unable to void voluntarily. The following day the bladder was washed with 10 cc. of 0.5% aqueous mercurochrome as an additional

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therapeutic measure, and this also failed to relieve her.

Since this patient had failed to respond medically for a period of 12 days, it was decided to use hypnosis. Accordingly, in the presence of the floor nurse, the patient was placed on a bedpan. The top of the bed was elevated to an 80 degree angle so that the patient was in a semi-erect position, with her back lying comfortably on the elevated head-part of the bed. She was then placed in a light trance. She was reassured in the induction that no one would have any charge over her, that the nurse who was present in the room did not have any charge over her, that the physician did not have any charge over her and that everything that would occur would be the result of her choice.

Because of her emotional resistances, no statements which could arouse antagonism were made. All statements made to her were carefully worded with a "See if you can't" phrasing. She was told to picture herself in the bathroom with her bladder full and with an eagerness to void and that she would be greatly relieved. She was assured that there was no pathology, that her urethra was now open with no obstruction and that all she now had to do was to make pressure in her abdomen and thus void. Accordingly, with the first effort the patient voided freely with a sighing statement, "What a relief."

She was now told three things: (1) She would remember everything that was told to her to the effect that she would be able to void voluntarily at will whenever she so decided; (2) every time that she would desire to relax, she could do so; (3) that she would now open her eyes fully relaxed, with a very clear head, feeling fully satisfied and happy. There followed no further difficulty.

HYPNOSIS IN A SPECIAL GAGGING SITUATION

by Normajane Wittner-Barrett, D.D.S.[1]

The following case history suggests the usefulness of hypnosis in a specific trying situation and the favorable effect upon both the patient and the doctor.

In August 1959 a patient, approaching his seventieth year, requested a new upper denture because, due to gagging, the original impression had been faulty and had resulted in an unsatisfactory plate. It also had to be trimmed excessively on the posterior border.

The preliminary or "snap" impression for the new denture was obtained under the difficulties of gagging. At the following appointment a choice was offered the patient: repetition of the unsatisfactory previous attempts or the use of hypnosis in obtaining a better impression for the subsequent wearing of a denture of the proper length.

Fully appreciating the problem which he faced, the patient expressed a willingness to be hypnotized. He developed hypnosis quickly, produced glove anesthesia, and upon suggestion transferred that anesthesia to his mouth and soft palate. He gagged slightly while testing with a tongue depressor, so he was told to return some of the anesthesia to his thumb, then to replace his thumb upon his palate until the palate was adequately numb. He retested himself with the tongue depressor with no more gagging. He was told emphatically that the numbness would remain for the rest of the appointment to meet his needs for taking his impression. No suggestion was made concerning the future denture or appointment.

The patient was then aroused, and the impression was easily taken. As he was leaving the office, he volunteered, "I don't believe I'll have to gag any more." When he returned for the completed denture, which was felt to be longer than actually necessary, he insisted upon keeping it the length it had been made, saying, "Everything is going to be all right." In this statement subsequent events proved him right.

[1] 31 South Park Street, Hanover, New Hampshire.

WHAT IS THE VALUE OF HYPNOSIS IN CHILDBIRTH?

by Walter Fening, M.D.[1]

Is hypnosis in childbirth of sufficient value to compensate for the additional time required for training the patient and the attendance of the physician during the labor? Every doctor who uses hypnosis can cite cases in which the labor and delivery were almost ideal, but how often does this prevail? We read of the widespread interest in hypnosis among the laity, who incidentally are chronically interested in any new method that promises to be a panacea for the pains of childbirth. Many articles written by physicians for both lay and medical journals are lavish in their praise of the method, but I have seen few which tell of the number of failures and partial successes. I think that we need more information, dealing with the problems that obstetricians face in their daily practices. I feel quite inadequate at times, considering that my poor results may be due to lack of skill on my part, but, on the other hand, do others obtain better results? I do not know.

I am sure that I am more skilled with hypnosis than I was 2½ years ago when I first started to use it but, as will be shown later, my percentage of good results has not improved. Feeling that the literature on hypnosis needs evidence of the success and failures by the average practitioner as well as the inspirational papers of the leading authorities, I would like to present my results, using hypnosis in childbirth over the past 2½ years.

In my locality, I have been the first and until very recently the only physician using hypnosis in practice. There is much interest in hypnosis among the laity, as evidenced by frequent requests for speaking engagements at mothers' study clubs, home-maker circles, and social groups, which I have fulfilled. However, these talks have enlightened me probably more than my audiences. In the lengthy question-and-answer sessions that follow, it becomes evident that many are hoping that hypnosis will provide a miraculous, passive key to a painless labor and delivery; others are disappointed that many of the unpleasant symptoms attending pregnancy, such as nausea, heartburn, and nervousness cannot consistently be removed; and still others are convinced that it is a fine thing but not for them. I have tried to impress very forcefully upon my audiences that hypnosis is becoming an increasingly important medical tool and that people should overcome their prejudice and try to view it as simply that.

Quite a few patients inquire about hypnosis during their prenatal visits, and here let me state that I find it far wiser to let patients request training in hypnosis rather than have me suggest it. Some of these lose interest upon my introductory discussion, and others decide against continuing with training after one or two sessions. Their reasons usually remain secret, since I do not try to probe their rejection of hypnosis, but some confess to fears of failure, family disapproval, or superstitious anxiety. It is somewhat unfortunate, at least from a statistical point of view, since some of these patients who discontinue their training have already proved to be good pupils. Finally there remains a small group of patients who have utilized their hypnotic training with varying success through their labor and delivery.

RESULTS

Twenty-five patients form a very small series on which to base any conclusions. However, I feel that some of my problems will be instantly recog-

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nized by other physicians and may be helpful to them in their practices. During the 2½ years covered by this study, my associate (who does not use hypnosis) and I have delivered about 1200 patients. Therefore, about one in 50 patients has utilized hypnosis successfully or unsuccessfully for childbirth. This is really not as poor as it seems at first glance, when one considers that in my town we are only eight years removed from drop-ether anesthesia administered by the one and only nurse, and that nowadays the suggestion of spinal block for delivery does not instantly bring a loud protest, as it did just a few years back. However, all these excuses still do not alleviate the feeling that perhaps I haven't been very successful in "selling" hypnosis.

Of the 25 patients in this study, one must be eliminated at the start; though she was progressing rather nicely during the first part of her labor, she was medicated heavily by my associate, who had forgotten that she was to try hypnosis. She was too medicated to continue with her training instructions. Later she expressed regret and felt that she could have been successful.

I have divided the remaining 24 patients into three categories: (1) *Good*—those who received little or no medication throughout their labor, utilized a trance successfully, and afterwards felt that the entire procedure had been an enjoyable experience. (2) *Fair*—those patients who required more medication and who had objectively more discomfort or apprehension than those patients in group 1. (3) *Failed*—those patients who became very apprehensive and upset or loudly demanded medication.

The latitude of each group is wide enough, so that there was no question as to classifying an individual's performance. I do not think that success should be measured by the use or non-use of chemical anesthesia. My criteria for the successful use of hypnosis in pregnancy are minimal requirements of medication during the labor and the sense of enjoyment and accomplishment throughout the entire procedure. Delivery without chemical anesthesia adds nothing to the safety margins for the mother and the baby, and so I place little or no emphasis on this. Other desirable but not essential effects are postpartum perineal anesthesia and the absence of after-cramps.

In the early portion of this study, I attempted to elicit many different hypnotic phenomena in each patient, but with accumulating experience I have narrowed a patient's training to three elements. First, I attempt to teach them glove anesthesia and strongly suggest that this will be directly proportional to their analgesia during labor. Most are able to accomplish this fairly well. Secondly, learning to accept posthypnotic suggestion is more difficult, and fewer patients can accomplish this. Lastly and probably most important of all, they are given repeated suggestions that labor and delivery can be a pleasant, enjoyable, and spiritually elevating experience. I am even experimenting with a decreased number of training sessions at present to see if better results may be forthcoming, since several patients in the past exhibited increasing resistance with numerous practice-sessions.

Of the 24 patients, 15 achieved a rating of *good*. Some of these required small doses of medication, most were given local infiltration for episiotomy, and a few had spinal blocks. All were quite happy with their experience and felt it worth while to have taken the necessary training. Six patients were rated as *fair*; they received more medication and were obviously more uncomfortable and apprehensive than the patients who were rated *good*. Some of these patients were intrinsically more skilled in hypnosis than some in

the first group, but for reasons such as prolonged labor, a hysterical reaction of a patient's first delivery which was very traumatic, etc., they had more difficulty. Three patients were classed as *failures*. One of these was the most skilled patient I have attended, but her failure must have had a deep psychologic significance for her, since later she was very anxious and depressed, feeling that God had punished her with failure for a lack of religious faith.

Dividing the patients into two chronologic groups, the *early* delivering before Jan. 1, 1959, and the *late* delivering after Jan. 1, 1959, we find that 8 of 13 patients were rated as good in the late group and 7 of 11 patients achieved this classification in the early group, thus indicating that my operative skill either did not improve or was not significant in the number of successes.

Correlation between a patient's ability with hypnosis and her subsequent performance could not be made, since there were several very interesting problems that interfered in a deleterious way.

SUMMARY AND DISCUSSION

Of 24 patients, 62 per cent achieved a satisfactory degree of comfort and enjoyment during their childbirth, utilizing hypnotic techniques. Six patients were only fairly successful and three patients failed.

In view of the considerable amount of time expended for the prenatal training and the necessity of presiding at the bedside during the labor, I wonder if I am achieving sufficient success to warrant continued use of the method or whether my results indicate a lack of skill on my part.

Do other physicians who, like me, practice in regions that are slow to accept hypnosis, achieve a greater percentage of successes, or is 66 percent about the maximum that we can expect? Is there a definite percentage of patients who are not able to utilize hypnosis in childbirth successfully, just as there is a definite percentage of people who cannot enter a somnambulistic trance? These are questions that I would like to have answered.

In conclusion, I would like to assert that those patients who were able to utilize hypnosis successfully received far more satisfaction and enjoyment than those patients in whom chemical analgesia and amnesia were successfully obtained.

P. Aboulker, L. Chertok, and M. Sapir. La Relaxation. Aspects Théoriques et Pratiques. 2ème édition augmentée. (Relaxation. Theoretical and Practical Aspects. 2nd revised edition.) Paris: Expansion Scientifique Française, 1959. viii + 154 pp. 900 Fr.

By André M. Weitzenhoffer, Ph.D.

This work, published under the auspices of the Société Française de Médecine Psychosomatique, consists of a collection of 14 articles by various authorities, for the most part European, dealing with a number of psychotherapeutic techniques subsumed under the broad topic of "relaxation." Among the topics discussed are: a variety of techniques, including traditional hypnotism, which have been used in close association with "painless childbirth," Schultz' autogenic training and a variation of it developed by de Ajuriaguerra, Jacobson's progressive relaxation, E. Kretschmer's active fractionated hypnosis, and Stokvis' active regulation of muscular tonus. These various techniques range from placing a minimum of emphasis upon the interpersonal aspect of therapy to giving the latter a major place. With perhaps Jacobson's methods as the one exception, the remainder of the techniques described in this work, according to their adherents, involve the production of hypnosis or of some state closely akin to it. As the title of the book indicates, much of the material in it is of a practical nature and descriptive of the methods. There are some exceptions to this, such as one article concerned with electroencephalographic data suggesting a relationship between hypnosis, the yoga trance, and the effects of autogenic training, and another article concerned with certain psychoanalytic aspects of relaxation under its various forms discussed in this book.

It should be clear from the outset that whatever "relaxation" comprises here, it is much more than one would expect from the consideration of a standard dictionary definition. In fact the term seems to have been given such a broad coverage as to become relatively meaningless. Chertok, one of the co-editors, apparently recognizing this fact, has made a valiant effort to return some specificity to the expression, but not too successfully in this reviewer's opinion.

As Chertok points out, traditionally the term "relaxation" had specific and unique reference to a muscular phenomenon, a release of muscular tension. In more recent times, however, there has been added the implication of a release of psychic tension. Alone or in combination, these two kinds of release of tension have been considered to have therapeutic value per se. There are many ways of attaining such a release of physical and psychic tension, ranging from the presumably purely physical to the presumably purely psychological approach. Whatever differences the various techniques discussed in this book may have, they have one point in common — at some stage or other they all lead to a state of "relaxation" which has therapeutic value. Whether or not all the methods lead to the same over-all condition in each individual, whether they are to be considered as essentially various forms and variations of a basic type of manipulation of individuals, remains to be seen. The electroencephalographic data reported in the work are by no means definitive in this regard, and it is to be noted that the book as a whole makes no pretense to answer this and related questions.

Although the articles outline, often quite well, the *modus operandi* of the various techniques under consideration, one can hardly consider the book as an adequate textbook. Its main value, as this reviewer sees it, lies in the fact that it brings together and describes concisely and rather well a number of specialized techniques of psychotherapy better known and more extensively used in Europe than in the United States. It is a fine source from which to familiarize oneself with these techniques, but any person wishing to utilize them is strongly advised to seek other and additional sources of information.

The various chapters cannot be said to be of uniform quality. Most are quite good, but some are mediocre. Jacobson's contribution is disappointing and adds little to the work. Held's discussion of relaxation techniques in relation to psychoanalysis is rather superficial and appears to be more speculative than based on facts.

It is also to be regretted that the editors and authors did not see their way to furnishing better statistics than the few which are offered, thus to allow one a better appraisal of the relative merits of the techniques which are described. Many of the arguments given in favor of one or the other method seem to be more in the form of academic and theoretical considerations

than practical ones. Of course we have the assertions of the various adherents of each method, who invariably feel that their past personal experience shows beyond question the superiority of their preferred therapeutic approach. Although such testimonials are certainly of value, they cannot replace adequate statistics. There are no reasons to doubt that in the hands of the various authors contributing to the present work the techniques which are described have given excellent and perhaps even remarkable results. This, however, seems generally true of any psychotherapeutic approach. In the hands of the proper therapist and applied to a properly selected group of patients, each and every one leads to outstanding results. Insofar as the relaxation approach is concerned, one thing seems clear. The techniques which are described will be found for the most part to be disappointingly slow and even tedious by the hard-pressed American practitioner and the time-conscious American patient. One ought not to overlook an important possibility in considering the adoption of any one of the methods which are described, and this is that they may be better suited to the European temperament and way of life than to the American temperament or life-style. When everything has been said, this reviewer remains unconvinced of the superiority of the various other relaxation techniques which are discussed over suggestive hypnotic methods. Hypnotism, as understood today in America, may still offer, when properly used, the quickest and most effective way of obtaining therapeutic relaxation in the American culture. This, of course, is but an opinion and, like any other opinion, remains to be supported by adequate statistics.

In final evaluation, the present work makes for interesting and useful reading and ought to serve well as an introduction to a group of potentially useful psychotherapeutic techniques. Undoubtedly the fact that this volume is in French will limit its American audience. It is to be hoped that a translation will soon be available.

R. K. Webb. *Harriet Martineau: a Radical Victorian.* New York, Columbia University Press; London, William Heinemann, 1960. Pp. xii + 385. $5.

By Frank A. Pattie, Ph.D.

This book is a biography by a professor at Columbia University of Harriet Martineau (1802-1876), an English journalist and advocate of reform who wrote on political economy, religion, and her travels in the United States and the Near East. She wrote many books, their subjects ranging from children's tales to the government of India. She was a Unitarian Christian who later became a free-thinker and a positivist.

The author devotes one chapter to her mesmeric treatment and her espousal of the cause of mesmerism. She collapsed in 1839 as a result of a prolapsed uterus and polypous tumors and remained an invalid for five years, suffering greatly. The tumors were removed, but no surgery was done for the prolapse. She had begun to improve somewhat when mesmeric treatment was started in 1844. After five months of treatment, administered by two mesmeric practitioners and by her own maid, who had watched the experts and produced excellent results, she was able to walk as much as five miles a day. She became enthusiastic about the benefits of the treatment and threw all her energy into advocacy of this very controversial method of treatment. Her case became famous. It was written about by Miss Martineau and others. She believed, on inadequate evidence, that a young girl in her household had clairvoyant powers while in a trance, and she accepted phreno-mesmerism, the linkage of phrenology and animal magnetism.

Her contacts were with Elliotson and his followers. She apparently did not know James Braid, who rejected the theory of an impalpable fluid held by the mesmerists and explained the phenomena by means of physiological and psychological principles.

The author gives an excellent panorama of the controversy in the 1840's on supernormal abilities in the trance, the religious implications of mesmerism, phreno-magnetism, and on the very reality of mesmeric phenomena. He discusses the internal conflicts in the group of mesmerists and between the two journals, the *Zoist* and the *Phreno-Magnet.*

In his comments on the history of mesmerism in the 18th century, the author states that the Academy of Medicine appointed the commission of 1784. This is not correct. The Academy of Sciences, the Royal Society of Medicine, and the Faculty of Medicine of Paris all had refused prior to 1781 to accede to Mesmer's request for systematic investigation, and it was the King who appointed the commission in 1784. Another correction is the title of Puységur, who was the Marquis de Puységur and not the Count.

Miss Martineau's death was the result of a large ovarian cyst. The author does not mention any use of mesmerism by her in

her later years nor anything about a change in her attitude toward it. Probably nothing was to be found on this subject, as her interest had perhaps declined along with the general loss of interest in hypnotism which followed the deaths of Elliotson, Esdaile, and Braid. Curiously enough, the author does not tell the date of Miss Martineau's death beyond saying that she died "just two months short of the centenary of the death of David Hume."

T. W. Frost. *Hypnosis in General Dental Practice.* Chicago: Year Book Publishers. 1959. Pp. 128. $4.50.

By Jerome B. Albert, D.M.D.

This book is offered as a handbook and guide for dentists who have had training in hypnosis, and it stresses much of the "help and reassurance" that can be derived from the experienced practitioner, such as the author. By way of introduction the rationale, uses and application, and some of the phenomena of hypnosis are discussed. A verbatim record is given of an entire patient-dentist situation which is instructive and informative of the British approach to the development of rapport. Discussion is given of standard techniques of induction and the usual theories of patient reaction, together with an exposition of various tests as alleged trance deepening procedures. Specific applications of hypnosis in dentistry are discussed and specific case histories are cited.

All in all, the book is a good but rather average presentation of unoriginal material.

ABSTRACTS OF CURRENT LITERATURE

Edited by Bernard E. Gorton, M.D.

(The letters P.A. after an abstract indicate that it is reprinted with permission from *Psychological Abstracts.*)

75. Johnson, W. R., Massey, B. H., and Kramer, J. F. Effect of posthypnotic suggestions on all-out effort of short duration. *Res. Quart.*, 1960, **31**, 142-146.

Ten young men in excellent physical condition were trained to meet specified criteria of hypnotic trance depth (including complete posthypnotic amnesia) and were tested twice on all-out rides of 100 revolutions on the bicycle ergometer (26.8 lb. resistance). The subjects were placed in a trance before both test rides, but before only one test were they given posthypnotic suggestions, which were not recalled consciously, that they would have unusual strength, endurance, and freedom from fatigue and would recover rapidly after the exertion. The mean advantage of the ride following posthypnotic suggestions was not statistically significant, but the subjective reports of the subjects were favorable to the suggestions. (A.M.W.)

76. Shor, R. E., and Schatz, J. A critical note on Barber's case study on "Subject J." *J. Psychol.*, 1960, **50**, 253-256.

A critique of a case study reported by Barber, in which a previously excellent somnambulist had allegedly become unable to respond to hetero-hypnotic situations. An experiment was performed to demonstrate that this subject is still able to respond satisfactorily in the classic heterohypnotic somnambulistic manner. (A.M.W.)

77. Levitt, E. E., and Persky, H. Relation of Rorschach factors and plasma hydrocortisone level in hypnotically induced anxiety. *Psychosom. Med.*, 1960, **22**, 218-223.

Plasma hydrocortisone was determined under hypnosis and following a hypnotically induced anxiety state in 12 normal subjects. Rorschach tests were simultaneously administered. Three of 26 Rorschach factors were found to correlate with change in PHC level. The three factors were: Pure C (color response), P (popular response), and P (percentage of good form response). The first was a positive correlation, the other two were negative. Five other Rorschach responses known to be elevated following hypnotically induced anxiety states and which are indexes of neurotic anxiety did not correlate with hormonal level. The authors suggest that the appearance of the three prepsychotic indicators is dependent on an increased level of circulating hydrocortisone. (A.M.W.)

78. Lerner, M. Fenomenologia de la induccion hipnotica. [Phenomenology of the hypnotic induction.] *Bol. Brasil. Soc. internac. Hipn. clin. exper.*, 1958, **1**, #3-4, 540-548.

The suggestions of the operator polarize the subject's attention, resulting in a narrowing of the sensory field. Simultaneously muscular relaxation is accentuated. Muscular tonus, which through proprioception is one of the most important factors for the integration of the ego, relaxes to a point such that an ablation of the ego-boundaries takes place. As a consequence profound changes in body image result. In this condition, many affective manifestations can take place (crying, aggression, anguish, sexual excitement, etc.). Usually, however, the induction of hypnosis is pleasant and can produce by its sole effect a cessation of pathology (asthma, migraine headaches, etc.). All of the personality planes, conscious and unconscious, come into action during the induction, and as hypnosis progresses the initial dissolution of the ego gives place to a reintegration of the latter in a new plane, in which the operator image is intrapsychic. The therapeutic effects of the hypnotic induction per se can be ascribed to (a) psychosomatic relaxation and (b) strengthening the subject's psyche by including in itself the operator image as a source of power and security. The eye-fixation method produces first an oculocervical tension, which then gives way to a reflex of generalized relaxation and

drowsiness, all of which favor hypnosis. The author suggests there are anatomical and physiological reasons for the eyes having a special position in the induction of hypnosis. Stage techniques are compared with medical hypnosis. The author takes the position that theatrical hypnosis is harmful and dangerous, while medical hypnosis is not. (A.M.W.)

79. Schneck, J. M. The hypnotic state and the psychology of time. *Psychoanal. Rev.*, 1957, **44**, 323-326.

Hypnotic subjects may seek an obliteration of time beyond mere perceptual distortion. There is a deep wish to control time. The hypnotic phase is regarded as an experiential void. Trance and sleep are associated with death. The Sleeping Beauty myth is cited illustratively. (P.A.)

80. Fahler, J. ESP card tests with and without hypnosis. *J. Parapsychol.*, 1957, **21**, 179-185.

Four subjects were tested for ESP in the waking state and again under the influence of hypnosis. The scores in the waking state were near mean chance expectation, while those in hypnosis were positive and statistically significant. No conclusion is reached regarding whether the improved scoring was due to hypnosis as such or to heightened motivation. (P.A.)

81. Chertok, L. Über die Entwicklung der psychologischen Analgesie in der Geburtshilfe. [The development of psychological analgesia in obstetrics.] *Psyche*, Heidel., 1957, **11**, 543-556.

There are presently two hypnosuggestive methods for producing analgesia during childbirth: (a) Dick-Read's (1953) and (b) Velvovski's (1949). While the methods are similar and both enjoy relative success, their theoretical bases are incomplete and contradictory. In addition, the problem remains of accounting for differences among individuals' responses to these methods. There is a great need for studies of criteria to predict the degree of painfulness of childbirth and its reducibility through verbal methods. (P.A.)

82. Kenitz, C. F., Jr., and van Scotter, D. A. The application of hypnosis in optometry. *Optom. Wkly.*, 1958, **49**, 475-476.

The use of hypnosis as an aid in fitting contact lenses, in tonometry, and other areas of optometric practice is illustrated by short case histories. (P.A.)

83. Ratner, S. C. Hypnotic reactions of rabbits. *Psychol. Rep.*, 1958, **4**, 209-210.

Each of 4 young rabbits was placed 24 times in a V-shaped trough and occurrence of hypnotic reactions (rho) observed. Results show that rho can be induced in rabbits and consistently terminated with auditory stimulation (hand clap). While tension does characterize rho, the results are interpreted in part as a function of or a by-product of fear. (P.A.)

84. Popek, K., Vagner, B., and Dostal, J. Untersuchung der Beziehung beider Signalysteme des Menschen mittels der Suggestion unter Benützung der Amplitudeänderungen der Herzkontraktionen als Indikator. [Investigation of the relationship between both signal systems in man as indicated by amplitude changes of cardiac contractions under suggestion.] *Psychiat. Neurol. med. Psychol.*, Leipzig, 1958, **10**, 221-230.

The changes in cardiac amplitude during a suggested 400-yard dash were recorded in 22 young adults. This method revealed the presence and intensity of suggestibility. Pulse rate and heart size, which change in the same direction during actual races, responded independently and at times paradoxically under suggestion. The method failed to reveal the relationship of the two signal systems. (P.A.)

85. Barber, T. X. Hypnosis as perceptual cognitive restructuring: III. From somnambulism to autohypnosis. *J. Psychol.*, 1957, **44**, 299-304.

The author reports a case study as evidence that hypnosis involves trance behavior (relative detachment from reality-stimuli) and a readiness on the part of the subject to accept any statement of the hypnotist as a valid description of events to come. (A.M.W.)

BIOGRAPHICAL NOTES ON OFFICERS OF THE AMERICAN SOCIETY OF CLINICAL HYPNOSIS

EDWARD E. ASTON received the degree of D.D.S. from the Baltimore College of Dental Surgery, University of Maryland, in 1925. He is engaged in the practice of general dentistry in West New York, New Jersey, is married, and has a son and daughter of high school and college ages, respectively. Dr. Aston is a Fellow and the Vice-President of the American Society of Clinical Hypnosis. Other memberships which he holds are in the American Association for the Advancement of Science, the New York Academy of Science, the American Dental Association, and the Academy of Psychosomatic Medicine.

HERBERT MANN graduated from the College of the City of New York with a B.S. degree and in 1935 received his M.D. degree from the Cincinnati Medical College. He became interested in psychotherapy and hypnosis during the early years of his general practice. As a World War II battalion surgeon in the European Theatre he was afforded ample opportunity to apply hypnotic techniques in the treatment of anxiety states. After the war he became Assistant Professor in the Student Health Department of San Jose State College, then returned to private practice in 1949. Formerly he was Chief of Staff of Doctors General Hospital. Dr. Mann is a lecturer on the staff of Seminars on Hypnosis Foundation and was a faculty member of the Symposium on Hypnosis sponsored by the New York State Academy of General Practice at its 1959 Annual Meeting. He was program chairman of the Second Annual Meeting of the American Society of Clinical Hypnosis and is President Elect of the Society. He is the first President of the San Jose Society of Clinical Hypnosis, a Fellow of the Academy of Psychosomatic Medicine, and a member of the American Medical Association, American Academy of General Practice, American Association for the Advancement of Science, and the Society of Abdominal Surgeons.

SAM SHAFFERMAN received both his pre-medical and medical education at Emory University in Atlanta, Georgia, and served his internship at New York City Hospital. In the years 1944-1946 he served in the U. S. Army Medical Corps. After leaving the Armed Forces he entered private practice in Columbiana, Alabama. In 1952 he moved to Birmingham, Alabama, and in association with Dr. Milton B. Rubin is engaged in the general practice of medicine. He is a past president of the Birmingham Society of Clinical Hypnosis, and a lecturer on the staff of Seminars on Hypnosis Foundation. He is also a member of the American Academy of General Practice and a Fellow of the Academy of General Practice and a Fellow of the Academy of Psychosomatic Medicine. He is a Fellow and the Treasurer of the American Society of Clinical Hypnosis.

BIOGRAPHICAL NOTES ON ASSOCIATE AND CORRESPONDING EDITORS OF THIS JOURNAL

GEORGE H. ESTABROOKS was born in St. John, New Brunswick, Canada, in 1895. He received the B.A. degree at Acadia University in 1920, and from 1921 to 1924 he was a Rhodes Scholar at Oxford University. In 1926 Harvard University conferred on him the degree of Ph.D. Since 1927 he has been a professor at Colgate University. He is the author of several books: *Hypnotism, Man the Mechanical Misfit, Spiritism,* and (with Richard Lockridge) *Death in the Mind.* In 1960 he plans to publish with Nancy E. Gross *The Future of the Human Mind.* He is a member of the American, Eastern, and New York State Psychological Associations and President of the Mid-State Society of Clinical Hypnosis. He is a Fellow of the American Society of Clinical Hypnosis and of the Society for Clinical and Experimental Hypnosis. His present research interest centers in the use of hypnosis in the treatment of juvenile delinquency.

ISAAC GUBEL received the degree of M.D. in 1952 at the University of Buenos Aires. His specialty is psychotherapy. He has made numerous trips to Europe and North America, visiting the most important clinics. His educational activities include more than twenty courses in hypnosis, which he has given in Argentina, Colombia, and Venezuela. He is the founder and President of the Sociedad Argentina de Hipnoterapía, which has more than 350 members, and the Federación Latino-Americana de Hipnosis Clínica, which is an international group of 15 active societies in Latin America and Spain. This organization publishes the Revista Latino-Americana de Hipnosis Clínica. Dr. Gubel is an Honorary Fellow of the American Society of Clinical Hypnosis and of numerous South American societies.

RENÉ PENNA CHAVES was awarded the degree of M.D. by the Medical School of the University of Brazil. At present he is Professor of Experimental Psychology at the Catholic University of Campinas, Brazil, and Head of the Department of Psychosomatic Medicine of the Clinica Santo Antonio Hospital. He is the founder and the first President of the Sociedade Brasileira de Hipnose Clinica e Medicine Psicossomatica. Among the positions that he has formerly held are the presidency of the Sociedade de Medicina e Cirurgia of Campinas, Assistant Professor of Medicine of the University of Brazil Medical School, Assistant Professor of Pathology of the Rio de Janeiro University Medical School, and Professor of Histology and Microbiology of the School of Dentistry of the Catholic University of Campinas. He is an Honorary Fellow of the American Society of Clinical Hypnosis and an Honorary Member of the Sociedad Argentina de Hipnoterapía.

LEWIS R. WOLBERG, born in Russia in 1905, received the degree of M.D. from Tufts University in 1930. He was a resident at the Boston Psychopathic Hospital in 1932 and received post-graduate training at the State Hospital, Kings Park, N. Y., and the New York and the American Institutes of Psychoanalysis. He is at present Clinical Professor of Psychiatry at the New York Medical College. He is the author of *The Psychology of Eating* (1936), *Hypnoanalysis* (1945), *Medical Hypnosis* (2 vols., 1948), *The Technique of Psychotherapy* (1954), and numerous articles and contributions to collaborative works.

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